National Institutes of Health (U.S.) Office of the Director.

SPEECHES AND ARTICLES BY/ON DR. JAMES A. SHANNON

Volume IV

RA
11
D63305
1961-68
v.4

SPEECHES AND ARTICLES BY/ON DR. JAMES A. SHANNON (CONT.)
VOLUME IV

DATE	PLACE	TITLE	See TAB
7/26/66	Wash., D.C., U.S. Senate	Statement by Dr. Shannon before the Subcommittee on Government Research, Senate Government Operations Committee.	79
10/22/66 PUBLISHED	San Francisco, Calif.	"The advancement of medical research: A twenty-year view of the NIH role." Presented at The Ninth Alan Gregg Memorial Lecture of the 77th Annual Meeting of the Assoc. of Amer. Med. Colleges. Published in The Journal of Medical Education, Vol. 42, No. 2, Feb. 1967.	80
10/25/66	Oklahoma City, Okla.	"NIH--present and potential contribution to application of biomedical knowledge." Remarks presented at the Conference on Research in the Service of Man. (Harris Committee)	81
12/1/66	Boston, Mass.	"Medicine, biomedical sciences, and engineering." Presented at the Third Annual Meeting of the Amer. Institute of Aeronautics and Astronautics (Avco Everett Research Laboratory).	82
12/15/66	Wash., D.C.	"Statement by James A. Shannon, M.D." Statement delivered before Senator Muskie's Subcommittee on Intergovernmental Relations, Senate Committee on Government Operations, at hearings on S. Res. 298. (Muskie Committee)	83
12/19/66 ARTICLE PUBLISHED ON SHANNON.		"James Shannon--a vignette of a great 'bureaucrat.'" Published in Modern Medicine, Dec. 12, 1966, pp. 49-52.	84
12/31/66 ARTICLE PUBLISHED ON SHANNON.		"General of the war on disease." Article published on Shannon by Business Week, December 31, 1966, pp. 88-90.	85

DATE	PLACE	TITLE	See TAB
1967	Wash., D.C., U.S. House of Rep.	"DHEW, Statement by Director, NIH, PHS on 1968 appropriation estimates."	86
1/67 PUBLISHED		"National Institutes of Health." Published in Industrial Research, January 1967.	87
1/16/67	Wash., D.C.	"Science and service." Delivered at the Conference on Regional Medical Programs, Washington Hilton Hotel.	88
1/23/67 PUBLISHED		"Medicine moves toward systems techniques." Article published in Technology Week, January 23, 1967, P. 72.	89
2/22-24/67	Houston, Texas	"Role of fundamental research in a mission-oriented agency." Presented at the annual meeting of the Biophysical Society.	90
3/1/67	Wash., D.C.	"Statement by James A. Shannon, M.D., Director, N.I.H., before the Subcommittee on Government Research, Senate Government Operations Committee.	91
4/18/67 PUBLISHED TWICE	Chicago, Ill.	"Some further thoughts on the relationships between science and federal programs." Presented at the 51st annual meeting of the Federation of American Societies for Experimental Biology. Published under the title, "Thoughts on the Relationships Between Science and Federal Programs," in the Educational Record, Summer 1967 issue, by the Amer. Council on Education. Then also published under the title, "Relationships between science and federal programs," in Federation Proceedings, Vol. 26, No. 6, September-October 1967.	92

SPEECHES AND ARTICLES BY/ON DR. JAMES A. SHANNON (CONT.)
VOLUME IV

DATE	PLACE	TITLE	See TAB
6/19/67 PUBLISHED	Atlantic City, N.J.	"Some comments on the occasion of the 1967 Passano Award to Dr. Irvine Page." Read on the occasion of the presentation of the Passano Award, 6/19/67. Published in JAMA, July 24, 1967, Vol. 201, No. 4, pp. 109-110.	93
9/11/67	Chicago, Ill.	"Bioengineering aspects of medical research programs of the NIH." Co-authored by Drs. Shannon and Sessoms (delivered by Dr. Sessoms). Keynote address at the 22nd annual conference and exhibit of the Instrument Society of America.	94
11/21/67	Baltimore, Md.	"Academic science and the federal establishment." Presented at the University of Maryland School of Medicine.	95
1/8/68 PUBLISHED		"The research philosophy of James A. Shannon, M.D." Published in Scientific Research, January 8, 1968, P. 44.	96
1/12/68 PUBLISHED		"Federal research." Published in Medical World News, January 12, 1968, p. 98.	97
2/15/68	Detroit, Michigan	"The extended life span: health factors." Co-authored by Drs. Shannon and Sessoms. Delivered at Wayne State University.	98
3/14/68	Wash., D.C.	"DHEW, Statement by Director, NIH, PHS on 1969 appropriation estimates."	99
4/1/68	Wash., D.C.	"DHEW, Statement by Director, NIH, PHS on 1969 appropriation estimates."	100
4/15-16/68	Hempstead, N.Y.	French and Polyclinic Medical School and Health Center Seminar entitled, "Health Center of the Future." Seminar held at Hofstra University.	101

SPEECHES AND ARTICLES BY/ON DR. JAMES A. SHANNON (CONT.)
VOLUME IV

DATE	PLACE	TITLE	See TAB
4/18/67	?	President's Science Advisory Committee Presentation. An outline.	102
5/24/68	N.Y.C.	"College of Mount St. Vincent Commencement address."	103
1968, Summer PUBLISHED		"Medicine: improving the 'quality of living.'" An inteview with Dr. Shannon. Published in The General Electric FORUM for Exploring the Frontiers of Human Progress, Vol. XI, No. 1, P. 15, Summer 1968.	104
7/68	Wash., D.C.	"Statement by James A. Shannon, M.D., Director, N.I.H., prepared for hearings before the Subcommittee on Science, Research, and Development, House Science and Astronautics Committee.	105
8/27/68	NIH	"Hail and farewell."	106
1968 PUBLISHED		"Dinner address, The University Medical Center and Medical Education." Published in The Transactions of the Association of American Physicians, Vol. LXXXI, 1968.	107

TAB 79

Statement by
James A. Shannon, M.D.
Director, NIH

Before the
Subcommittee on Government Research,
Senate Government Operations Committee

July 26, 1966

Mr. Chairman and members of the Committee:

I appreciate the opportunity to discuss with you the role of the National Institutes of Health in strengthening higher education and academic capability throughout the Nation. A series of specific questions have been posed for me to answer. These may be regrouped as follows:

1. What are the issues which are involved in providing a broader geographic base for those elements of science and education that are our responsibility?
2. What steps have we taken to implement the President's memorandum relating to this and dated September 13, 1965?
3. Does our peer system of scientific review militate against achieving the objectives set forth by the President?
4. Finally, what has been our experience to date in implementing this directive?

I fully realize, Mr. Chairman, that the Committee is approaching the general problems of geographic distribution and academic development in a studied and systematic fashion. Therefore, I should like to approach

the presentation of the attitude, programs, and actions of the National Institutes of Health by first developing a quite broad base for our discussions.

Quite apart from the specific directive of President Johnson, dated September 13, 1965, the President and the Congress have previously made it clear in a variety of ways that they wish a more equitable geographic distribution of funds by all agencies concerned with support of science, including mission-oriented agencies. It is also stated both implicitly and explicitly that this should be accomplished without lessening the chances of mission-oriented agencies accomplishing the purposes for which they have been created and for which the Congress appropriates public funds.

We believe that the desire to accomplish such a complex objective stems from an appreciation by the Congress and the President that our society, all sectors and all geographic areas, is increasingly dependent upon the development of a very broad base of science and technology to provide for

1. Adequate educational opportunity for our citizens regardless of geographic origin;
2. An adequate technological base for industrial development in all regions of our Nation;
3. An adequate educational and scientific base for the medical and other professional groups that relate to and service the communities and citizens in all regions of our Nation.

These important objectives are further viewed as possible only if the educational enterprise in each region can provide for a broad range of scientific activities in both the fundamental and applied areas in physical sciences, mathematics and engineering and in the behavioral and biomedical sciences as the necessary scientific and technological base for the instructional requirements of:

...the prebaccalaureate experience,

...graduate education,

...continuing education.

The satisfaction of these requirements is deemed to be an imperative, since the quality of education and the quality of professional and industrial activity are perceived to be directly related to the degree that students, professionals, and industrial groups have easy accessibility to scientific and technological activities of high quality.

If one views these objectives broadly, then in the health field we must be concerned with the impact of a number of Federal programs other than those which simply support science. This conclusion then forces the view that for a full examination of these matters one must inquire into all programs which have direct bearing on

1. The opportunity for science generally to thrive in a given geographic area,
2. The development of a sound scientific base for the biomedical sciences in the universities, the medical schools, and the major hospitals in the area,

3. The development of programs of continuing education insofar as these prevent rapid obsolescence of professional groups delivering health services,
4. Finally, the extent to which such programs affect the quality of services delivered which in no small measure relate to the organization of services on a community level and the role Federal programs can play in the improvement of these.

Within the guidelines provided by the questions posed by the Committee, I will largely limit my remarks to those areas of our programs which impinge directly upon the support of a scientific base for medicine. However, my answers to these questions would not be meaningful in the absence of developing a frame of reference for my response.

The first point I would make is that I do not come before this Committee clothed in sack cloth and ashes to confess shortcomings on the part of NIH and its programs and with a commitment to do better in the future and to propose a whole new series of programs which will accomplish objectives which we agree with the Committee are highly desirable.

Rather I will review with the Committee the historical development of our programs with specific and special reference to problems of geographic distribution and to our mission to utilize science in the achievement of a solution to major problems of human development and human disease.

Our current programs had their major beginnings in fiscal year 1956. In the prior decade and with quite modest budgets, it had been possible to establish a sound conceptual base and effective mechanisms for the discharge

of limited parts of our mission. It had been possible to do this in a careful manner not particularly disturbed by social forces external to the field of science, largely because budgets had been small and time for development adequate. At the take-off point of our more recent development we had

1. A categorical structure grouping our activities around visible and generally understandable social objectives, namely, the solution of a series of major disease problems,
2. A strong, scientifically sound, direct research operation,
3. A broad program of research support through the mechanism of project grants awarded on the basis of a structured system of peer judgments,
4. The beginnings of a sound program to extend and broaden the scientific base of medicine through the utilization of fellowships and training grants.

Thus in 1956 an adequate base for rapid expansion had been developed, but the annual expenditure, which was barely $97.0 million a year at that time, was inadequate when viewed in relation to the social significance of the disease problems that were to be solved. However, it seemed that in the foreseeable future the limitation on the development of the enterprise should be limited more by manpower and facilities than by budgeted dollars in support of the scientific enterprises. It was therefore decided that the NIH should develop an energetic program to develop new resources,

both manpower and physical facilities. Budgetary provision was made to expand training activities, and new legislative authority and budgetary provision was sought to expand research facilities. These general concepts were found wholly acceptable by the Legislative and Appropriation Committees of the Congress. Shortly thereafter, explicit legislative authority was sought and made available, in the form of a new institution--oriented general research and training grant, to support institutional growth and to provide some measure of institutional stability as a balance to the project system of research and training support.

Now as to the explicit relacionship of some of these activities to the problem before this Committee, viz., the geographic distribution of scientific support. At the outset the NIH was more fortunate in this regard than other Federal agencies, since:

1. The primary education enterprise with which the NIH development was concerned was the medical school and its related institutions.
2. The extensive modification of medical schools and medical education during the 1920's and 1930's had provided the NIH with the beginning pattern of geographically dispersed scientific excellence to a far greater extent than the centers of graduate education and research in the physical sciences, engineering, and mathematics.
3. The state of knowledge of medicine was such that the major thrust of the research activity, whether fundamental or applied, involved primarily individuals, departments or institutions, in contrast to the R&D activity in the physical sciences which had

their primary thrust in areas of hardware and large-scale technology involving relatively few academic environments and emphasizing large development activities which had their primary expression in large industrial contracts.

4. The state of technical development in the biomedical field was such that the support of individuals or even modest size groups of scientists did not require, as was frequent in the physical sciences, large capital outlay.

5. Finally, with the growth of our population, there was a growing appreciation that the number of physicians serving our population must be substantially increased in the coming decades. The development of an added number of complete new medical educational facilities would provide an opportunity for these to be more equitably distributed throughout the Nation. These new centers would be staffed largely by young science-based professionals and would make it possible to achieve broader geographic distribution of funds but still maintain high standards of excellence and provide support in a fashion which would best serve NIH mission objectives, i.e., the solution of disease problems.

It is important to note that at that time, viz.,the late 1950's, it was generally accepted and explicitly stated by the staff of the NIH, our National Advisory Council, as well as our appropriation and substantive legislative committees, that our objectives were only achievable were it

possible to broaden the geographic and institutional base of our endeavor. In point of fact, this view was well appreciated from the very early years of our programs. Thus working within its mission the NIH has had a creditably constructive impact on the problem of geographic distribution.

In fiscal year 1965, the NIH obligated more than $642 million to 420 colleges and universities, with every State represented. This was for the support of health research, training, construction, and other programs aimed at expanding the national capability for scientific effort in the area of health. At this level of expenditure, NIH programs account for more than one-fourth of total support (and more than one-third of the research support) provided by major Federal agencies to institutions of higher education.[1/] Roughly one-half of this $652 million went for the support of research projects, and nearly one-half was allocated to the investment in science resources for the future: health research facilities, training and fellowships, and general research support.

Recent trends in distribution of NIH support for institutions of higher education reveal broadening of such support both geographically and by institutions. In 1952, for example, institutions in the South, Southwest, and Far West received 28 percent of NIH research grants; in 1965 institutions in these regions received 43 percent of the total.

Of greater significance are the changes which have occurred in the dispersion of NIH funds by institution. In 1965, 102 higher education

---

1/ Data excludes university-managed, Federal contract research centers.

institutions in 40 States received more than $1 million in NIH awards, as compared with only one such institution in 1952, 17 in 1957, and 68 in 1962. Further, the past five years have witnessed a 50 percent increase in the total universe of institutions receiving NIH research grants support. From a 1959 total of 283, the number of educational institutions receiving NIH awards rose to 421 in 1965.

The reasons for the relative breadth and comprehensiveness of this higher education support are clear. NIH programs, from the beginning, have recognized that effective support for research carries with it the obligation to enlarge the resource base for continued growth in the future. As a consequence, NIH programs have provided fellowships, have strengthened the structure of graduate education in health-related fields, have provided stable support for faculty expansion, and have stimulated the construction of health research facilities through a matching grant program. In this respect, these programs have led the way for other agencies in advancing the objectives of the President's directive of September 13, 1965. Consequently, I think it more relevant that my comments be addressed to the problems of how we can further the progress begun in the late 1950's rather than what we have done since October of last year.

Looking to the future, as noted above, the new medical schools emerging will further enhance our opportunity to provide increasing geographic distribution of funds and still maintain a program of high scientific excellence and one which holds scientific missions clearly in view. However, although the opportunities are there, I feel less secure at this time of accomplishing these objectives than I would like to be the case.

This view is taken because of a series of very real circumstances that could dominate our situation during the coming decade.

1. It seems likely that budget levels will be held below the minimum to maintain the strong current base and move progressively to encompass goals that are otherwise achievable.
2. There is some pressure for short-term goals relating to the application of knowledge to supplant the stable support of a developing science. The former will not be generally useful for institutional building.
3. Programs aimed specifically at institutional building and institutional stability have not gained high priority in budgets review process during the past three to four years.
4. We now find ourselves faced with a situation wherein the rate of increase of funds for the support of research cannot support a substantial number of newly developed young scientists, who are emerging from explicitly designed research training programs as well as from training obtained through participating in research projects.
5. We find ourselves unable to secure the opportunity for career development of a substantial number of our young scientists because of inadequate funding of our program of fellowships aimed specifically at career development.

You have requested comments on whether we can provide for institutional development and geographic dispersion of our support programs within budgets now available to us and at the same time accomplish the scientific mission of the agency. Thus, because of the circumstances noted above, the answer to this question must be no.

Your questions indicate a desire to know what is needed in our programs that would assure our achieving the combined objectives, i.e., achievement of mission and institutional development, taking geographic distribution as an important end in itself. The following would be helpful:

1. A statement of Congressional intent relative to their willingness to provide for some plan of increasing support for the coming decade that separates the support of individual scientists and their institutions from support of the application of knowledge in the delivery of services; i.e, what can the biomedical academic community look forward to as the basic attitudes of the Congress during the coming decade?
2. A statement of the extent to which the Congress will encourage and support the emergence of new centers of excellence in the biomedical sciences. Here I have specific reference to assurances that are needed by those who are developing some 25 new medical and associated institutions between now and 1975 which, in large measure, can be specifically developed to satisfy a need for geographic distribution.
3. A statement of the extent to which the Congress will encourage and support the development of excellence in as many as 20 to 40 existing educational institutions already producing approximately 100 physicians each, annually. This can be done by substantial expansion of our own capability for institutional development and through substantial expansion of the Health Professions Educational Assistance Act administered elsewhere in the Public Health Service.

Given these three assurances, expressed in part by a modification of existing authorities and supported by budget actions on the part of both the Executive and Legislative Branches, two practical and very desirable objectives will be achieved:

1. The security of opportunity provided for the young scientists will make him more mobile and more willing to move into young developing institutions. I am convinced that, above all other factors, tight budgets tend to freeze the bright young scientist in the institution of prestige.
2. State and local initiative will be unleashed to a degree and in places heretofore not experienced.

I am convinced that the intelligent and informed legislation underwritten by the Congress in fields of science and education has raised the aspirations of our citizens in all parts of the country to an extent that some years ago would have been believed impossible. And I say this not in a platitudinous fashion, but as the result of numerous working sessions we have had with professionals and citizens interested in the health field in every State of the Union during the past year. There is currently a substantial commitment to excellence in the health field that can be nurtured and brought to full flower in the coming decade--provided the Federal Government provides the stimulus and energizes the response by partial but adequate support of the undertakings.

I will close my remarks by saying I do not believe that anything of merit will be achieved by simple "sharing of the wealth of science support"

on any kind of a formula basis. I do not believe any program or plan of support is reasonable as a Federal action in the science field unless it requires as a counterpart a deep local commitment to excellence. To emphasize this point, I would say that a program that contains no science is better than a program characterized by poor science.

I further believe that the diversity of origins of support in our Government can be utilized in an imaginative fashion, with general agencies achieving their general objectives and mission-oriented agencies achieving their mission objectives in the common achievement of a geographically dispersed enterprise which would provide each region of the Nation with the scientific and technological base for aggressive educational, professional, and industrial enterprise if the budgetary support is forthcoming. As far as the health field is concerned, this is not wishful thinking but a demonstrated fact, inasmuch as this has been and is happening in many areas already.

TAB 80

#4.39

REPRINTED FROM
The Journal of MEDICAL EDUCATION
VOLUME 42 • NUMBER 2 • FEBRUARY, 1967

The Advancement of Medical Research: A Twenty-Year View of the Role of the National Institutes of Health*

JAMES A. SHANNON, M.D.†
National Institutes of Health, Bethesda, Maryland

In the invitation to deliver this lecture, it was suggested that I use the occasion to review the federal role in medical research over the past twenty years with a view to elucidating the present national scene and to assessing the issues of the future. This I shall attempt to do. I am aware, however, of the bias that can arise through intimate association with a course of events. One is apt to impute a degree of order and intelligence to a process that may in reality have been largely random, unintended, and opportunistic. If I do fall prey to this error, I shall appeal to an observation of Herodotus:

> Very few things happen at the right time, the rest do not happen at all; the task of the conscientious historian is to correct these defects.

THE MEASURE OF CHANGE

The quantitative measures of change which I should like to examine are familiar to us all. In 1947 the national expenditure for medical research totaled $87 million, of which the federal share was $27 million, or 31 per cent. The National Institutes of Health that year expended $8.3 million, or 10 per cent of the national effort. For 1966, national medical research expenditures are estimated at $2.05 billion.‡ The federal component is $1.4 billion, or 68 per cent; and the NIH portion amounts to $800 million, or 40 per cent of the whole.

A signal fact of this twenty-year comparison is that total dollars available for the nation's medical research have increased 24 times. If the Gross National Product deflation factor is applied to correct for price and wage changes over this period, the increase is still in the order of 15 times. The evidence of this change is clear. We see it in the expansion of research manpower and facilities; in the size of our graduate enrollments; in the extent and diversity of conferences, meetings, and publications; in the growing pace of scientific progress; and, unfortunately, in the burden of administrative mechanisms, forms, and detail. In this process our professional and administrative lives have been transformed and our relationships to the social and political order brought to a crucial point. I shall attempt to explore and assess

* The Ninth Annual Alan Gregg Memorial Lecture delivered at the 77th Annual Meeting of the Association of American Medical Colleges, San Francisco, October 22, 1966.
† Director.

‡ Exclusive of construction and training.

hese implications after probing the nature of the change that has taken place.

As we are well aware, the growth of ederal medical research programs has rought the changes we have experienced. In this twenty-year period the nnual federal support of medical research increased 50 times. Moreover, unding of NIH programs rose from 30 o 60 per cent of the federal expenditure. hus, it is the circumstances surrounding he growth and change in NIH programs hat contain the determinant influences nd the critical events of this remarkable appening.

THE PRECIPITATING CIRCUMSTANCES

The specific conditions which give rise o any major change in the course of uman events are always difficult to identify. Beginnings are lost in the interlay of many complex short- and long-ange variables. Thus, to delineate the nfluences which set the stage for the ostwar expansion of the medical sciences s, at the least, artful and perhaps presumptuous; at the worst, disingenuous. onetheless, reasonable men might agree hat the following matters were substantially contributory.

HIFT IN FEDERAL CONCERN

The transformation of the role of the ederal government in national affairs hich took place in the 1930's was without question the broad underlying event. or the first time in its history, the government reached out directly in a time of eace to affect the social and economic ecurity of the people of the nation in a assive way. This shift of national concern and governmental action set the cene for the great events we have known.

ATIONAL CANCER ACT

The first directly significant event involving NIH and the support of medical esearch was the passage of the National ancer Act in 1937. This legislation reated a national institution centered upon the problem of cancer—an institute authorized to conduct research in its own facilities, to provide grants-in-aid for nonfederal research projects, and to award fellowships for advanced study and training, all under the guidance of a National Advisory Cancer Council comprised of nonfederal leaders in science and public affairs.

In this extraordinary, imaginative, and far-sighted legislation, the shape and ingredients of the future for the medical sciences were cast.

PUBLIC HEALTH SERVICE ACT

The intervention of World War II directed national attention and energies to the world struggle. The promise represented by the National Cancer Act was thus delayed. The means were there, however, for exploitation in more favorable times. And the ill winds of war were not without some good. Limited national health activity provided the occasion for a thorough overhaul and codification of the laws under which the Public Health Service operated. This culminated in the enactment of Public Law 410, the Public Health Service Act of 1944. A significant component of this new law was Title III, providing broad authority to the Surgeon General to foster, conduct, support, and cooperate in research relating to health and disease. Authority to award funds for nonfederal research and research training, hitherto limited to the field of cancer, was extended to health problems generally.

FEDERAL-UNIVERSITY ASSOCIATION

Another major benefit deriving from the war period was the association of the federal government with universities in the furtherance of wartime research problems. The Office of Scientific Research and Development (OSRD) was set up to coordinate and contract for this work. Its activities clearly demonstrated the contribution that university science could make to the solution of national problems. It also established the first set of terms and conditions relating to university-federal relationships, which have led directly to the present-day pattern.

PROMISE OF SCIENCE

The awesome detonation over Hiroshima imbued us all with a sense of what technology, built upon the results of basic science, could accomplish through massive effort. In like manner the nation was impressed with the emergence of penicillin and the other war-accelerated "wonder drugs." These generated high expectations of science as a major instrument for the solution of national problems. Thus, the nation emerged from the war disposed toward generous support of science and its institutions.

STATE OF BIOMEDICAL SCIENCES

It was through the purposeful and perceptive support programs of the private foundations that the biomedical sciences had rapidly developed in the first four decades of this century. Building upon a foundation of European basic effort, American biomedical scientists made key contributions to microbiology, biochemistry, and physiology. These advances were coupled with growing technical and research capability in the areas of medicine, surgery, and instrumentation. The impetus of war-related problems brought this capability to impressive potential. As a consequence, the end of World War II found American bioscience ripe for major expansion. Ideas were burgeoning. A large pool of professional manpower was ready and awaiting the opportunity for independent work.

There was, however, an important difference from the prewar period. The ravages of the depression years, the rising tax burden, and the complex demands of modern science had steadily eroded private financing of medical research. If the promise of the biosciences was to be realized, it would have to be at levels only possible through public support. But to gain support for expenditures of this magnitude, the relevance to public needs and wants would have to be clear and sharp. In this context the research programs of the voluntary health agencies rose to prominence, presaging the great success of the National Foundation for Infantile Paralysis. The public need and want, however, were greater than could be met by the voluntary approach alone. Governmental initiative and resources were required.

THE CRITICAL DECISIONS

It was in this setting that the biomedical developments of the immediate postwar period unfolded and the pattern for support of science was established. The future course of medical research turned upon a series of critical decisions.

OSRD TRANSFER

With the termination of the wartime Office of Scientific Research and Development, arrangements were made for the transfer of the remaining university contracts in the biomedical area to the National Institutes of Health. Similarly, the university contracts in the physical sciences were transferred to the Office of Naval Research (ONR). The presumptive purpose was to provide for the orderly phasing out of these war-born support activities. The decisions reversing this intention—decisions to continue support by both NIH and ONR—were probably the most influential and far-reaching in the history of federal support for university research. They were made at a time when the country was attempting to define the federal government's role in the fostering of scientific endeavor. When the National Science Foundation Act was passed in 1950, the residual OSRD programs of NIH and ONR had been absorbed into a firm base of

expanding mission-oriented research programs, ending the concept and the prospect that NSF would be the principal source of support for U. S. science, either basic or applied.

Biomedical research was now linked to the research component of the Public Health Service—the National Institutes of Health.

EXPANSION OF THE CATEGORICAL CONCEPT

The concept of an all-out national attack upon a major disease problem through a categorical institute came to full maturation in the late 1940's. The Congress passed the Mental Health Act in 1946. The National Heart Institute and the National Institute of Dental Research were authorized in 1948. Broad legislation, known within the Public Health Service as the "Omnibus Act," was passed in 1950, empowering the Surgeon General to establish institutes to deal with major disease problems. The National Institute of Neurological Diseases and Blindness, the National Institute of Arthritis and Metabolic Diseases, and the National Institute of Allergy and Infectious Diseases are consequences of this legislation. Thus, the "National Institute of Health" (as it was known from 1930 to 1948) became the plural "National Institutes of Health." The categorical concept had come to full fruition. Through these categorical institutes, the public desire to defeat the major diseases was transformed into the basic and applied scientific effort necessary to achieve that end. This was a decision uniquely American, and one that has greatly accelerated the advancement of science and medicine. These latter events are in no small measure attributable to the foresight of Leonard A. Scheele, Surgeon General from 1948 to 1956.

USE OF THE NONFEDERAL COMMUNITY

Until the postwar period, virtually all NIH research was carried out in the intramural laboratories at Bethesda and related field stations. The grant-in-aid, though authorized initially for cancer in 1937 and broadened in 1944, had accounted for a total expenditure of only $1.5 million from its inception through 1945. The impetus of the OSRD program transfer, plus the new categorical legislation, soon changed this pattern of expenditure. In 1955, 3,300 research grants and 1,900 training grants and fellowships were awarded in the amount of $55 million, almost 70 per cent of the total NIH appropriation for that year. The broad involvement of the nonfederal research community with national objectives for the conquest of disease was now well under way.

Early in the postwar period, another farsighted decision was made: the role of the National Advisory Council was buttressed by the establishment of panels of experts to judge the scientific merits of research proposals. This was the beginning of the Study Section review and priority rating system, which has ensured the quality and relevance of the research supported through NIH programs. Indeed, the character and quality of a significant part of the national biomedical research effort has been the consequence of a unique process for reaching a consensus of the scientific community involved. The actions of these past twenty years might well be viewed as the product of a "republic of science."

SUPPORT OF FUNDAMENTAL SCIENCE

Still another decision made early in the development of NIH programs was that the Congress, in view of the state of knowledge concerning health and disease, must be persuaded to lend broad support to the sciences fundamental to medicine. Acceptance of this vitally important concept has been reflected in NIH appropriations over the years. A recent expression of the Congress's endorsement of free-

ranging, fundamental research is the following statement from the Senate Appropriation Committee Report on the Labor-HEW Appropriation Bill for 1967:

> It was clear to the committee, as a consequence of the extensive testimony of both public and private witnesses, that both the current state of medical knowledge and the changing character of medical and health problems required substantial, sustained, and unrestricted commitment to the free ranging individual research efforts selected for support through a process of reaching a consensus of the scientific community on the excellence, significance, and relevance of the intended work. A major proportion of the increased appropriations recommended by this committee over the years has been in support of this commitment. The committee continues to be convinced that progress of medical knowledge is basically dependent upon full support of undirected basic and applied research effort of scientists working individually or in groups on the ideas, problems, and purposes of their selection and judged by their scientific peers to be scientifically meaningful, excellent, and relevant to extending knowledge of human health and disease.

INVESTMENT IN RESOURCES

In the mid-1950's a series of decisive steps was taken, largely as a consequence of the intelligent and farsighted leadership of Marion B. Folsom, then Secretary of Health, Education, and Welfare. The first of those steps was the Congress's acceptance of the principle that medical research in the national interest, though costly, should not be restricted by lack of funds. Beginning in 1956, Mr. Folsom supported a series of budgetary increases for NIH which substantially reduced the deficit in funds available for high-quality research projects. Mr. Folsom also made vigorous efforts to obtain legislation providing federal matching grants for the construction of medical and dental facilities for research and teaching. The portion of the legislation concerned with teaching facilities failed passage at that time because of the Congress's inability to reach a consensus on the role of the federal government in education. Credit is due Mr. Folsom for the 1956 enactment of Public Law 84-835, the Health Research Facilities Construction Act, which has contributed so much to the nation's research capacity in the life sciences.

With equal wisdom, Mr. Folsom encouraged expansion of the research fellowship and training grant programs as an investment in the resources necessary to meet the needs of the national research effort. These early actions involving appropriations for construction and training were augmented by new programs for the support of specialized research resources, the development of research careers, and the general support of research and training in the interest of the institutions themselves. In the aggregate these actions underwrote a national medical research system commensurate with the promise and capability of science and the urgencies of the major disease problems.

SHIFT OF INITIATIVE

Despite the enlightened leadership of Secretary Folsom, there was no unanimity in the Executive Branch of government at that time concerning the federal role in the advancement of medical research, and no firm conviction that expansion was warranted. This ambivalence, reflected in less than imaginative appropriation requests, provided a clear opportunity for the Legislative Branch to seize the initiative. Two extraordinarily able and dedicated members of Congress, both with deep and long-standing commitment to national health support, rose to the occasion. Senator Lister Hill and Congressman John E. Fogarty, through brilliant use of the appropriation hearings process, amassed evidence of research needs and opportunities in relation to major disease problems and won broad congressional

concurrence for substantial increases. These 2 men and their colleagues, despite a more perceptive Executive attitude, have since maintained a dominant influence in determining the levels of federal support for medical research. The nation owes much to their courage, devotion, and skill.

THE DERIVATIVE PROBLEMS

Viewed in retrospect and in brief, the critical decisions of the past twenty years tend to assume an all-too-sweet air of reasonableness and simplicity. What is missing is the substance and force of historical reality, as well as the plain fact that serious problems continue to exist. Some of these are new and some have merely changed shape and character. I should like to explore the main ones.

BALANCE BETWEEN RESEARCH AND EDUCATION

A clear national consensus on the federal role in support of research in educational institutions was reached early in the postwar period. By contrast, the evolution of the federal role in education, and specifically medical education, has been attended by prolonged and bitter controversy. Not until the passage of the Health Professions Educational Assistance Act in 1963 was the barrier to federal action in this area penetrated. The initial legislation provided grant assistance for the construction of teaching facilities only. Not until 1965 was there assistance for the medical education process as such—in the form of general and special improvement grants—and this at what must be considered minimum levels. Thus, the better part of twenty years' growth took place in a setting which offered substantial federal aid for the research function of medical schools and universities, but no direct federal aid whatever for their basic educational role.

As pressures mounted on the educational side, serious imbalances and instability developed within many institutions. Inevitable distortions appeared in the conduct of research and training programs and the distribution of faculty effort. Fortunately, the authorization of direct support for education will help correct distortions due to the biased pattern of the past. It will not, however, modify the basic fact that the balance of functions within institutions of higher learning has changed.

ADAPTATION OF INSTITUTIONS AND INVESTIGATORS TO THE GROWTH OF RESEARCH

Research as a major academic function is a relative newcomer to the university scene. Even up to the immediate prewar period it was precariously supported for the most part from a variety of private sources or from the residuals of general operating funds—as a relatively minor, though important, adjunct of the higher educational process.

Today, research is a major and often dominant feature of the academic landscape. The financing of research and the education of scientists affect the whole stability and capability of many institutions of higher learning across the full scope of their functions. Both the institutions and the scientists have had difficulty in adjusting to this new order. Modification of the framework of academic organization and administration, which traditionally reflects the dominance of the teaching role, has been cautious, even in the face of rapidly expanding research functions. This finds expression in a persistent ambivalence concerning the extent to which research is an academic function supported by public funds or a public function housed in universities. Such uncertainty greatly diminishes the clarity and force which universities bring to their own planning and development, their exercise of administrative responsibility, and their negotiations with research-supporting agencies.

For the investigator, this transition in the source of support and the growth of university research functions emphasizes

a troublesome reality: the "institutionalizing" of science. For better or worse, the pursuit of scientific objectives now requires access to large-scale, complex resources, diverse technical skills, and collaborative scientific relationships attainable only in an institution capably administered and substantially underpinned. There seems to be a certain reluctance among some scientists to embrace the new conditions of scientific life and to comprehend fully the obligations that such an environment imposes. This state of immaturity is delaying the development of effective opinion within the scientific community on many matters relating to obligations of public support, the role of university administration, and the exercise of collective responsibility within science.

PUBLIC ACCOUNTABILITY AND THE TERMS AND CONDITIONS OF SUPPORT

The sevenfold expansion of federal medical research expenditures over the decade prior to 1962 has generated complex and unprecedented issues of public policy. Never in the nation's history had public funds in such amounts been placed at the disposal of individuals working in support of their own objectives outside the framework of federal institutions. There was ample evidence that neither the recipients nor the public and its representatives fully comprehended the essential conditions of this association.

For the scientists, there was the certain prospect that their support in one form or another would derive in growing part from public funds, thereby increasing in various ways their obligations to the public interest.

For the nation and its federal administrators and legislators, there was the difficult realization that the achievement of important national objectives involved ultimate dependence upon intellectual capability and creativity—delicate but vital qualities, the cultivation of which involves complex and frequently abstruse considerations sometimes contrary to traditional patterns of public function and often at odds with conventional public business.

The challenge to the federal science agencies and the academic science community, therefore, is to meld the considerations of national purpose with the conditions essential for scientific productivity while maintaining the integrity and independence of nonfederal institutions. As John W. Gardner, Secretary of Health, Education, and Welfare, noted in a recent speech before the consultants and advisors of NIH: "This is not an entirely easy assignment."

INSTITUTIONAL RESPONSIBILITY

Throughout the first fifteen years of NIH program growth, emphasis was placed upon meeting the urgent needs of its grantees and assuring optimum conditions for their productivity. Implicit in these arrangements was clear dependence upon the institutions housing the research to assure the proper use of public funds and to maintain the conditions of accountability. These terms and conditions, however, had not been uniformly articulated. This inadequacy was becoming apparent to all as NIH appropriations neared the billion-dollar level.

This was the setting in which the Intergovernmental Relations Subcommittee of the House Committee on Government Operations, chaired by Congressman L. H. Fountain of North Carolina, initiated its study of NIH activities. Seizing upon an incident involving the apparent misuse of grant funds by a quasi-nonprofit-making organization, the Fountain Committee undertook a searing inquiry into the administration of the NIH research grants programs. The consequence was a major clarification of the responsibilities of all parties involved. The promulgation of the new terms of this relationship—in regulations and in policy and

procedure issuances—provided a uniform basis for an understanding of the terms and conditions of support. The full measure of responsibility imposed upon grantee institutions for the effective and prudent use of grant funds was brought sharply into focus.

Most universities, and schools within universities, have realized the critical importance—to them and their faculties—of their capacity for sound administration. The faculty members who devote time and effort to increasing the ability of their institutions to govern themselves are performing an important service for science and its academic matrix.

Unfortunately, the proliferation of procedural requirements to meet changing problems has introduced a growing condition of rigidity and detail into federal support programs. This has tended to change the grant from its original character—a federal contribution to a nonfederal function in the public interest—to the *quid pro quo* relationship of the contract, with its burdensome accounting and auditing overlay.

THE CONFRONTING ISSUES

Looking to the future from this background of twenty years of growth, with its complex interplay of social, scientific, and political forces, one can speculate upon the major issues confronting the further advancement of medical research and education. I would select the following as being the most urgent of attention and the most determinant of our destiny: first, the conditions for the advancement of medical knowledge must be maintained; second, terms and conditions of support which assure effective achievement of the federal purpose must be devised; and third, medical educators and medical education must reflect a close engagement with the current urgencies.

MAINTAINING THE CONDITIONS FOR THE ADVANCEMENT OF MEDICAL KNOWLEDGE

In the context of tightening federal budgets, the nation's medical research effort will undergo increasing scrutiny. Searching questions are being directed toward the validity of current levels of expenditure, the priorities for further increases, and the balance between short- and long-range objectives in the conquest of disease.

This was the setting for the meeting of the President and the Directors of the National Institutes of Health in June of this year [1966]. In a forthright manner, the President expressed concern over the extent of research on specific health problems and the degree to which knowledge resulting from federally supported sciences was being rapidly applied in curing and preventing disease. He warned that the solution to the problem did not lie merely in the expenditure of more money, but must be sought through better distribution of effort.

These questions are both valid and proper. NIH and, indeed, the academic and scientific community must respond to them in a way that, on the one hand, reflects understanding of the vital conditions for progress in medicine and, on the other, demonstrates the relevance of current and intended attacks on the major health problems.

We must devote the necessary time and effort to convey to the literate, informed public, to our legislators, and to those who exercise executive decision over the allocation of resources the nature and conditions of progress in the medical sciences. It must be clearly understood that:

1. Knowledge of life processes and of phenomena underlying health and disease is still grossly inadequate. In the absence of broad general theory, such as exists in the physical sciences, the development of diagnostic, therapeutic, and preventive capability will continue to be dependent upon empirical approaches, serendipity,

and the intuitive brilliance of too few gifted individuals. Therefore, the hope of major advances lies in sustaining broad and free-ranging inquiry into all aspects of the phenomena of life, limited only by the criteria of excellence, the scientific importance, and the seriousness and competence of the investigator.

2. Contrary to general impression, the whole scope of the medical and related sciences is pervaded by a purposeful concern for achieving mastery over the hazards to human life and health. Research in the biomedical sciences contains a high component of practical and problem-oriented activity, though there is no clear visibility as to the extent of this activity. Unfortunately, research with practical objectives is too often equated with organized national programs such as cancer chemotherapy, vaccine development, or drug trials. It must be emphasized that such ventures are the exception rather than the rule. In the present state of our knowledge, there are limited opportunities for highly organized research of a national nature with specific short-range goals. Both types of venture must be very carefully selected, for they will be expensive and, if undertaken in the absence of an adequate scientific base, may be unproductive and hence wasteful of limited resources.

3. Despite the limitations of the base of biomedical knowledge, progress in our medical capability has been substantial. The benefits of this progress, however, are not universally available. This is evident in the differential between the quality of medicine offered in the nation's great centers of scientific and academic medicine and in too large a proportion of services at the community level. The solution lies not in medical science but in medical economics and sociology. In this area the private sector has heretofore been dominant. But the critical nature of these problems has generated growing public concern and action. If we are to preserve the private character of medicine in this country, the problems must be attacked with greater imagination, more concern for the public interest, and more willingness of diverse interests to cooperate than has characterized the private scene thus far. The NIH Regional Medical Programs provide a unique opportunity for such a reformation in medical service.

DEVISING EFFECTIVE TERMS AND CONDITIONS OF SUPPORT

Federal expenditures for research, training, and construction at institutions of higher education now exceed $3 billion a year. It seems of critical importance to identify the distinctive purposes of such federal expenditures. Two broad objectives are manifest.

One is the expenditure of federal funds in the procurement of academic services which are necessary for the accomplishment of federal business. The establishment of large-scale research centers, such as the Lincoln Laboratory at MIT and the Jet Propulsion Laboratory at Cal Tech, and the procurement by federal agencies of research testing and evaluation services in the conduct of diverse programs are illustrative of this broad area of federal need. The support is generally contractual in nature, involving a *quid pro quo* relationship and resulting from the requirements of government.

The other general objective is to support graduate research and education in broad fields deemed important to the public interest. This relationship, embracing a considerable portion of graduate research and education, constitutes a joining in common purpose. Federal funds are provided to assist in the conduct of a nonfederal activity which, though not requisite to the conduct of public business, is essential to the promotion of the common welfare and the achievement of national objectives in

such fields as health, education, and the advancement of science.

A critical question arises: Is this distinction in the federal use of universities sufficiently real and sound to provide a basis for clear distinctions in the terms and circumstances under which federal funds are made available? A corollary question is this: Given the basis for a differential in the policies and conditions of the relationship, can a meaningful and operational distinction be made in choosing the grant versus the contract as the instrument for such federal expenditure?

We at NIH have felt strongly that such distinctions are not only possible but vital. On the one hand, we have attempted to use the grant-in-aid as the instrument by which the government extends assistance to a nonfederal activity which is clearly in the public interest but which is not the conduct of a federal function. Its use does not modify the nonfederal responsibilities for the quality, direction, or conduct of the supported activity. On the other hand, we have tried to use the contract as the instrument by which the federal government obtains goods, services, and other considerations needed for the conduct of the federal business. The performance and obligations of both parties are specified in detail, and the responsibility for administration of the contract rests mainly with the federal agency involved.

Unfortunately, there has not been widespread agreement on this view of the federal relationship and the feasibility of reflecting it in differential instruments and in terms and conditions of support. The consequence has been an increasing dominance of the contractual concept in federal policies for the support of university functions. We have all been aware of the proliferation of restrictions in the use of federal funds under Public Health Service grants. We are approaching a condition that has been described in a recent book as "the contract state," in which there is a growing federalization of the entire university community. We believe this to be unwise and dangerous—a trend calling for careful and critical examination by the nation.

It seems evident from these considerations that federal agencies must carefully distinguish among the respective roles they play in relation to the universities and scientists. Particular vigilance is necessary to avoid confusing these roles and their respective instruments. Certainly, an important preliminary is some concurrence and expression by the Congress of its intent with regard to the roles of federal agencies in support of research and the statutory stipulations relating to grants and contracts.

Moreover, it is most important for the universities and scientists, in negotiating with federal agencies for support of research, to consider carefully whether they are seeking assistance in the performance, extension, and enhancement of their missions or whether they are offering assistance in the performance of a federal function. These distinctions will enable the parties involved to assure appropriate terms and instruments of relationship. Their application will do much to provide a sound, rational base for the inevitable extension of public support for university and scholarly activity. This clarification could also reverse the erosion of the more liberal relationships possible under grants.

INVOLVEMENT OF MEDICAL EDUCATION WITH URGENT ISSUES

I shall now attempt, at the risk of exceeding my role and competence, to express a few thoughts on a broader theme. In the past two years the Congress has passed legislation related to health which twenty years ago seemed impossible of attainment. The Heart, Cancer and Stroke Amendments of 1965, the addition of Titles XVIII and XIX

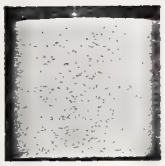
to the Social Security Act, and the extension of the Health Professions Educational Assistance Act providing operating support for schools and scholarships for medical students are among this array of new statutes. This achievement is reflective of the growing national consensus on the urgency and validity of federal action in support of medical education and medical science. Further health legislation is pending.

The needs and opportunities presented by these new programs, in parallel with the further advance of biomedical knowledge, will exert profound stress upon medical schools and our traditional concepts of medical education. I believe this circumstance imposes a set of imperatives upon all concerned with the future course of medical education, the medical sciences, and medicine. Encouraged by the bold and innovative spirit of the great man for whom this lecture is named, I shall express my views of these imperatives and the nature of the action I believe necessary to meet them:

1. The curriculum of medicine must reflect the progress of science, the changing phenomena of health and disease, and the essential conditions of sound graduate education. To the certainties of death and taxes can be added the inexorable advance of knowledge. A medical curriculum which emphasizes principles and concepts centered on the study of the phenomena of life rather than didactic disciplines centered in separate departments will provide a productive and continuing base of understanding for the student in the further course of education, research, and the practice of medicine. The effective expression of this concept will require, as the Millis Commission (1) so forcefully stated, the encompassing of the whole scope of medical training — predoctoral, internship, and residency—into an integrated process of graduate education pervaded by unified qualitative criteria.

2. The processes and institutions of medical education must meet the requirements for substantial numbers of additional physicians as well as preserve the qualitative standards. Society's increasing requirements for physicians is indisputable. A dramatic indication of the need is reflected in the licensing of mounting numbers of foreign-trained physicians in the United States—more than 1,500 last year. We are dependent upon the uncertain qualities of diverse systems of foreign medical education for almost one fifth of our new physicians annually. At the same time we are rejecting half of the qualified applicants for U.S. medical schools because of limited admission capacity. At this rate some 125,000 college graduates seeking careers in medicine over the next ten years will be rejected for admission to U.S. medical schools. In the face of our urgent needs, on the one hand, and the growing body of potential students, on the other, expansion of the classes of existing schools seems unavoidable. An immediate national examination of the feasibility of this action is imperative.

3. The national system of medical education, in addition to meeting the nation's needs for quality and quantity in medical education, must also act to sustain the level of professional capability of physicians in practice. An inescapable consequence of the advance of medical science is an accelerating rate of professional obsolescence of physicians. In my view the only answer to this problem is a formal and mandatory process of continuing medical education. I believe our plans for medical schools, and our estimates of medical manpower requirements for the nation, should embrace the concept that every sixth year of the practicing physician's career should be spent in a formal educational process. There should be public support for this procedure, and continued licensure of a



physician should be contingent upon his participation.

4. The process of medical education and the professional development of the physician must provide broad understanding and insight into the nature of social problems and the behavior of man. Whether as a consequence of medical education or of the forces of the practicing environment, physicians have not been distinguished by their views upon the urgent problems of a changing society. By the nature of his profession, the physician encounters the very essence of the human struggle, yet seems to reflect a public image of lack of understanding and concern for the social and political manifestation of human needs. A broader role for the behavioral sciences in medical education and emphasis upon the public responsibility of physicians are to my mind essential in the design of new plans for medical education and service.

CONCLUSION

In these remarks I have tried to cover the main events of the postwar history of NIH—an era marked by the most sweeping changes in the support of medical research that this century has seen. We have come a long way in a few short years, but the distance that stretches before us is equally great. The gains we have made must be sustained; the programs we have hopefully launched must maintain their momentum and their meaning.

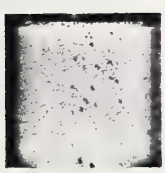
There is always need in science, as in other pursuits, to combat complacency; and few perhaps were more keenly aware of this than certain philosophers of the eighteenth century. I am reminded here of Voltaire's observation on the state of medicine in his time: "Doctors," he said, "pour drugs, of which they know little, for diseases, of which they know less, into human beings, about whom they know nothing."

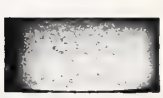
Regrettably, Voltaire's scalding words still describe much of medicine today—a fact from which we derive no comfort. What does encourage us is the knowledge that the questing spirit, which in all ages has found expression in men such as Alan Gregg, remains a central force for advancing science and bringing its benefits within the reach of all men.

REFERENCE

1. MILLIS, J. S. (Chairman.) *The Graduate Education of Physicians.* Report of the Citizens Commission on Graduate Medical Education. Chicago: American Medical Association, 1966.

#6017





TAB 81

NIH--PRESENT AND POTENTIAL CONTRIBUTION TO APPLICATION OF BIOMEDICAL KNOWLEDGE*

James A. Shannon, M. D.**

Senator Harris and members of the seminar:

I am pleased to participate in this discussion of "Research in the Service of Man." I should like to congratulate Senator Harris and the Frontiers of Science Foundation of Oklahoma on providing a productive setting for a broad examination of an important series of questions relating to the advance of biomedical knowledge and on extending the benefits of this discussion to the nation.

We who serve in the Washington scene have been particularly impressed with the intelligence, vigor and fairmindedness with which the Senator has directed the inquiries of the Senate Subcommittee on Government Research into the complex and difficult question surrounding the development of constructive policies bearing on the conduct of Federal science programs in relationship to national needs and objectives. We are also aware of the important role that the Frontiers of Science Foundation of Oklahoma is serving in encouraging and supporting an enlightened engagement with science and research in the Oklahoma area. Vigorous private action at the State and local levels is an essential complement to the conduct of national programs.

The preparation of this discussion required that a choice be made between considering some important general issues or the substantive programs of our several Institutes. I chose the former as the most appropriate for me, since a number of the substantive issues will be dealt with by individual scientists actively engaged in the fields they will represent.

In respect to the matter at hand today, I speak, as you will recognize, as the head of an agency which has served a very significant role in the advancement of biomedical knowledge. The role and purpose of the National Institutes of Health in this development is misunderstood by some. NIH is too frequently perceived as a science agency

---

*Remarks presented at the Conference on Research in the Service of Man, Oklahoma City, October 25, 1966.

**Director of the National Institutes of Health, Public Health Service, U. S. Department of Health, Education, and Welfare.

interested in biological and medical problems in themselves, rather than as a health agency utilizing science and research in the war on disease. But permit me to put to rest any uncertainties that may exist in regard to this matter. NIH exists primarily to deal with health problems.

Three major factors play a determining role in the form, content and mechanisms of action of NIH and its programs: (1) the rising social demand to diminish the hazard of serious disease, (2) the state of development of the biomedical sciences, and (3) the inherent nature of scientific advance.

First, the rising social demand to diminish the hazard of the major diseases. This force has been given major expression in the several legislative acts creating the categorical disease institutes which now form the basic structure of NIH. These acts have been complemented by generous and growing budgets in support of the institute programs and by the establishment of other program authorizations essential for the success of the enterprise.

Second, the state of development of the biomedical sciences. The base of knowledge of life processes and phenomena underlying health and disease is still grossly inadequate. Consequently, the development of any diagnostic, therapeutic and preventive capability is still largely dependent upon what are basically empirical approaches, the movement of science in collateral fields, the accidents of serendipity, and the intuitive brilliance of too few gifted individuals. As a consequence of those hard realities, effort has had to be directed, on as broad a scale as resources would permit, to advancing the base of science upon which disease-oriented research efforts are entirely dependent. Thus, a series of derivative missions--the operational imperatives to mounting a comprehensive attack upon health problems--has become an essential part of NIH activities.

I should like to emphasize again that these are indeed derivative from, but none the less indispensable to, the discharge of the primary mission of this agency. These activities encompass the following:

. . . The broad support of undifferentiated and nonprogrammatic research in the universities, medical schools, hospitals and other research institutions through grants-in-aid, and the conduct of research in the laboratories and clinics of NIH at Bethesda and its field stations.

. . . The training of specialized manpower through fellowships and training grants to provide for our future teaching and research capability on which continued progress in health depends.

. . . The construction of research facilities and the further development of the scientific capabilities of academic and research institutions.

. . . Finally, the conduct of collaborative and directed research programs aimed at specific disease or scientific problems where opportunity for a major advance in knowledge seems clearly possible and where circumstances and urgency warrant an organized and managed effort.

The discharge of these several responsibilities must be through mechanisms which provide the competent scientist some security of opportunity and the institution within which he resides a sense of institutional stability. The mechanisms utilized must also be such as to enhance rather than weaken the educational environment.

These are the integral and vital components of our program for the scientific conquest of disease. Our concern with the problems of bringing research progress to bear upon health practice and the delivery of services is evident in the newly inaugurated Regional Medical Program and the Community Mental Health Center Program. The Regional Medical Program, enacted into law just a year ago, will have an increasing primary impact on the quality of medical services available to the nation.

Thus, the pursuit of our primary mission has had, as a necessary corollary, the building of a productive national medical research system with broad and expert capability. Nonetheless, there is the clear obligation to reach deeply and continuously for opportunities to exploit advances in knowledge where this can be achieved by directly organized activity.

The third determinant factor in the conduct of NIH programs has been the inherent nature of the process of scientific advance and the conditions for the solution of disease problems.

The mechanics of scientific progress in medicine are complex, diverse, and often determined by unpredictable and seemingly unrelated events.

. . . The emergence of broad generalizations relating to particular phenomena.

. . . The direction of these generalizations to the study of a particular problem.

. . . The intensive and sharpening pursuit of promising leads to solutions.

. . . The appearance of a feasible course of action which may result in a useful result or end point.

. . . Concentrated effort for the practical development of a program so perceived.

. . . The broad application of the end product of the research to medical health practice.

This sequence is fraught with uncertainties and is often visible only in hindsight. Moreover, the precipitating events often emerge from unforeseen developments in a wholly uncontrollable manner. The dominant factors in this process are the internal logic of science and the intrinsic purposeful nature of the medical sciences. Research in the medical sciences is pervaded by a concern for achieving mastery over the condition of, and hazards to human life. This circumstance assures a high component of practical and problem-oriented research.

The circumstances under which one intervenes in this natural process to accelerate or direct the course of action in a highly organized fashion is indeed a most critical consideration. One must proceed with caution, since the state of knowledge is such that there are limited opportunities for such deliberate action. Further, such intervention is apt to be expensive and, if undertaken in the absence of an adequate scientific base, is likely to be unproductive and thus wasteful of limited resources. Frequently too, programmed activity, but not programmed research, must be undertaken where the science base is perceived to be inadequate.

Such selections must be made, however; and as our understanding of the biological and behavioral bases of human health and disease increases, the opportunities for these selections will inevitably increase. Consequently, the functional organization of NIH must reflect this need and contain such a capability.

To illustrate this process of scientific advancement and the problems associated with purposeful intervention to accelerate or direct the course of action, I should like to review the background and events which led up to what all acknowledge as one of the triumphs of postwar medical research--the development of the polio vaccine.

One could utilize with equal validity the development of open-heart surgery, of blood-pressure-lowering agents, or of modern antimalarials. Each of these case histories would have much the same characteristics as that of polio vaccine. My choice to discuss the latter example stems from the relative simplicity of the concept of a preventive agent, the

start-and-stop phenomena which were dependent upon the course of development of collateral science, and finally, the possibility to view in retrospect a series of judgments--some correct, some in error.

Altogether, I believe this case provides an experience for all of us which can be expressed in certain general lines of guidance for application to any program being considered for large-scale or intensive development.

The development of a vaccine, in outline form, involves five general steps, as portrayed in figure 1*.

In the case of polio, these steps had the time sequence indicated in figure 2. The question may be posed, Why the long delay--1909 to 1955--between agent identification and general use of a vaccine? A simple answer is that the science base for a definitive development was inadequate prior to the early 1950's and not totally adequate for some time later.

As shown in figure 3, the first attempt to produce a vaccine failed in 1935. This attempt had, as its science base, knowledge concerning (1) the specificity of antigens and antibodies, and the hazards of parenterally administered spinal-cord tissue; (2) a beginning understanding of viruses in the causation of disease, and the polio virus as the specific cause of paralytic poliomyelitis; (3) primitive information on virus modification--by attenuation as early as 1884 (rabies) and by inactivation as early as 1911 (rabies); and (4) one animal model system requiring the use of monkeys.

In retrospect the 1935 attempt was bound to fail because of an inadequate amount of fundamental information upon which to base the targeted program. The natural history of the disease was not fully understood, the complexity of the viral agents was not known, nor was the mode of transmission of the disease. Further, the crude suspensions of monkey spinal cord did not permit, with the then-current information, complete inactivation of infectivity and, at the same time, the retention of a capability of stimulating antibodies. Finally, simple safety tests could not be devised, and the hazard of allergic encephalitis due to monkey-cord preparations was not taken fully into account. By 1948 it was demonstrated that such immunization was possible in monkeys, a fact of fundamental but not practical use.

The next programmed effort was mounted in the late 1940's and had the benefit of much more information concerning the agents which produce poliomyelitis and about the nature of the disease itself. Equally important, several packages of information were available from other fields of biology which had developed concurrently but whose general objectives had no relation to the practical problems of poliomyelitis or its prevention.

---

*References are keyed to items in the figures and are grouped at the end of the article.

There was substantial increase in the sophistication of the general field of virology, not to mention the Rockefeller Foundation's superb work leading to yellow-fever viral attenuation by Theiler in 1937 and Francis's studies leading to specific inactivated-virus vaccines for influenza in 1945 (see figure 4). The greatest advances, however, were in animal model systems which permitted a progressive understanding of the general characteristics of viral diseases and the properties of their causative agents.

The most important single package of information directly relevant to polio was derived from the field of tissue culture (figure 5). This field had its beginning with Harrison and Carrel at the turn of the century as they applied to mammalian tissue the information derived from earlier studies of the growth requirements of bacteria in culture media. Some of these advances were due to Maitland in 1930, who showed that bits of tissue could be the basis of modest cell growth. His explorations were aimed at developing a more effective method for the production of the vaccinia virus.

The definitive work that permitted this field to move rapidly was done by Earle and his associates in studies of normal and cancer cells in tissue culture. They were trying to determine the biological and cellular characteristics of the cancer cell as contrasted with its normal counterpart. Their contributions, which covered a decade of effort, led to a capability to culture monolayers of cells. The technique was directly transferrable to the polio problem by 1949, and further studies on large-scale tissue culture techniques were transferrable to practical methods for viral production by 1953.

These later techniques, which made use of deep or suspended cultures, were dependent in no small measure on contributions from the fermentation industry, where evolving technology had produced (1) means for obtaining an array of organic chemicals by fermentation processes and (2) inexpensive techniques upon which the broad and important antibiotic industry is currently based.

The bodies of information which became available in the late 1930's, but particularly the 40's, were both numerous and definitive. They came about from the unorganized efforts of interested scientists, some supported by the National Foundation for Infantile Paralysis, which emerged as a significant factor in the late 30's, and some supported by the Federal Government.

The package of information (see figure 6) which was derived from the polio workers themselves was limited until 1939 by lack of small-animal models with which to explore many aspects of the disease. This need was

satisfied in part by the work of Armstrong and others. However, the diversion of scientific workers to the immediate tasks of World War II suspended progress for the next five to six years.

The most important act of the postwar period which led to the rapid evolution of the polio field was taken by the Foundation shortly after the war ended. After assessing the state of the art at that time, the decision was made that saturation support of the nation's best virologists was required so that, in addition to furthering the general field of virology, they could give some consideration to the special problems of polio.

The general appreciation that more than a single agent was involved in polio causation was resolved by a coordinated collection and study of approximately 1,000 viral isolations from poliomyelitis cases. It was determined that paralytic polio could be caused by agents which antigenically can be grouped into three, and no more than three, types. This was a monumental, highly coordinated task involving the use of thousands of monkeys, and was completed by Bodian in 1949.

Perhaps more important, Enders in the same year adapted the now advanced field of tissue culture to the problem of the study and production of viruses. This latter tour de force made a practical viral technology available for the first time to polio and other workers.

It was now feasible (figure 7) to mount a vigorous and purposeful program in the fields of diagnosis, pathogenesis and epidemiology of the disease, on the one hand, and vaccine development on the other. Both of these general fields advanced simultaneously, in part as a managed program, in part as independent activities lacking central management. In retrospect, each activity, whether directed or undirected, is seen to have been essential for ultimate success.

The choice in the vaccine development portion of this overall effort was to emphasize, coordinate and fund, again to saturation levels, the production of an inactivated viral vaccine, which came into being as a reasonable certainty about 1953 and into general use around 1955. As you know, this vaccine has been largely replaced by an attenuated viral vaccine of a later development that had its beginnings in the early 1950's and culminated in a generally accepted vaccine toward the end of the decade.

As I will point out later, the decision of the Foundation to throw its resources behind the development of an inactivated vaccine markedly increased the difficulties and greatly protracted the time required to develop the generally accepted polio vaccine we have today. Such acceptance is reflected best in the production figures of U. S. industry.

A recent analysis expresses the vaccine production as follows, in terms of immunizing courses produced: 1.3 million for the inactivated Salk vaccine, 14.0 million for the attenuated Sabin vaccine.

As I see it, then, there were three phases in the overall development of polio vaccine. I have outlined them in figures 8, 9 and 10, which reflect both scientific progress and administrative judgments.

Phase I included the following:

. . . Immunological means constituted a practical solution to the polio problem, 1933.

. . . Present knowledge was inadequate for solution of the problem, 1936.

. . . Techniques utilizing physical barriers (i.e., nasal sprays) were impractical, 1940.

. . . Finally, the general scientific base was judged to be inadequate for a programmed developmental effort, 1940 - 1945.

Phase II:

. . . It was decided that a program of saturation support for virology was indicated, 1946.

. . . Polio is caused by three agents, 1949.

. . . The phenomena of the disease were rather completely defined by 1952.

. . . Tissue culture advances provided a practical means for the solution of most of the virological problems, whether for disease diagnosis and study or for viral production.

Phase III:

. . . Vaccine development was now deemed possible, 1950.

. . . Success of vaccine development was too uncertain to lessen the saturation support for virology.

. . . The preferred technique selected for the vaccine development program was inactivation of the polio virus by formalin.

. . . By 1953 an effective vaccine seemed feasible. Accordingly, the field trial was undertake in 1954 and the vaccine put into general use in 1955. The problems that arose during the introduction of the vaccine were due to an inadequate science base.

. . . Some scientists believed that a better vaccine was possible through the techniques of viral attenuation. This latter vaccine, however, was delayed in its development due to an inability to conduct controlled studies within the United States.

Some of these judgments involved in the above course of events were made by the community of scientists, some by a support agency having a capability of modulating and hopefully accelerating the development of knowledge in a desired direction.

The essential steps by the Foundation related to their decision to provide broad support for virology (1946); their coordinated program of strain definition (completed by 1949); their decision that a vaccine development program was feasible (about 1950); selection of the inactivated virus as the preferred choice for a vaccine (about 1952); assurance of support for full development (about 1953); and finally, the judgments that the vaccine (inactivated) was ready for field trial and then distribution.

The consequences of this series of decisions made by the support agency were three.

1. The final inactivated vaccine that was distributed for general use required redesign of production methods and redesign of safety testing within weeks after its introduction. This demanded redefinition of the fundamental concepts upon which both inactivation and safety testing were based. In other words, the development had outrun its science base, and a substantial number of vaccine candidates were placed at serious risk.

2. The late observation that at least one simian virus (SV-40) was inactivated at a slower rate than the polio virus was not recognized for many months. This virus was a live viral component of an otherwise killed viral vaccine. Entering the vaccine production system via the monkey kidney tissue used in polio virus production, it was capable of producing tumors in experimental animals. Fortunately, once recognized, elimination of the hazardous virus in the production and testing process was relatively simple. SV-40 was one of a number of monkey viruses that were not recognized as potential hazards at the

time the inactivated vaccine came into general use, and its discovery as a potential hazard is another example of how the vaccine development program outran its science base.

3. The selection of the inactivated product as the viral agent of choice greatly delayed what now appears to be the definitive product, the Sabin attenuated viral vaccine. The delay was in part a problem of funding parallel developmental activities. But more importantly, the availability of a reasonably effective vaccine (Salk) precluded testing the effectiveness of the newer attenuated vaccine in the U. S., for this would have meant withholding a proven agent from a portion of the susceptible population. It was for these reasons, and with substantial initial criticism by U. S. scientists, that Sabin arranged for his initial field trials in Russia and Koprowski for his trials in Africa.

The major decisions that permitted the successful development of the currently accepted vaccine were two: (1) It was decided in the immediate postwar period to provide saturation support for virology of an undirected nature, and (2) it was decided that vaccine development of an inactivated agent was to be financed by additional funds and not by a diversion of funds then available for the support of undirected activities.

Now--to return to science of today--to its Federal support and the capability of Federal agencies, particularly NIH, to intervene in the normal development of a science field in order to accomplish its specifically defined objectives:

. . . The categorical structure of NIH is well suited for such intervention, and continuously holds the disease problem before the participating scientists.

. . . Whereas the rapid development of NIH support from 1956 to 1964 was largely devoted, as an immediate goal, to developing a broad, sophisticated base for academic science, taking into account the health and vigor of both the scientist and the institution in which he resides, NIH has long been responsible for managed programs aimed at the solution of important health problems.

. . . There has been an increase in programmed activity in recent years, and if the broad scientific base is as effective as we believe it to be, there will be an increasing number of such opportunities, but hopefully with continued support of undirected and undifferentiated science.

. . . The undifferentiated growth of the biomedical sciences, with its mix of applied and fundamental, has a strong internal logic. If this is interfered with, there must be a full appreciation that (1) the goal is important, (2) the science base is adequate or can be made adequate as part of the organized effort, (3) the losses which may accrue from mounting the programmed effort are counterbalanced by the prospective gains, and (4) developmental work is in itself a hazardous process, at times costly of dollars and manpower and without assurances of success.

. . . In the past our various institutes have performed with varying degrees of excellence in accepting and developing major advances of a practical nature. On balance, and from a less than objective point of view, I believe overall performance has been excellent but can be improved.

. . . Finally, the health and vigor of the biomedical sciences, their coupling with the educational process, their extension geographically into areas of scientific deficiency, and indeed the progressive emergence of new practical opportunities--all these depend on the maintenance of a broad and balanced base of undifferentiated science.

Figure 1

USUAL VACCINE DEVELOPMENT

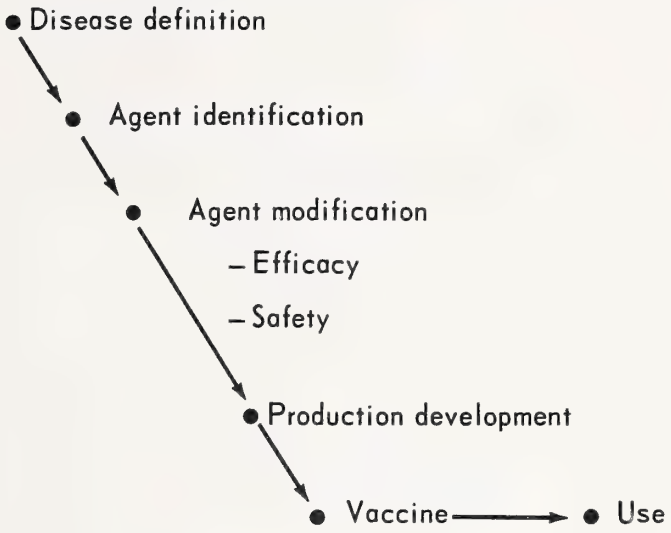


Figure 2

POLIO VACCINE DEVELOPMENT

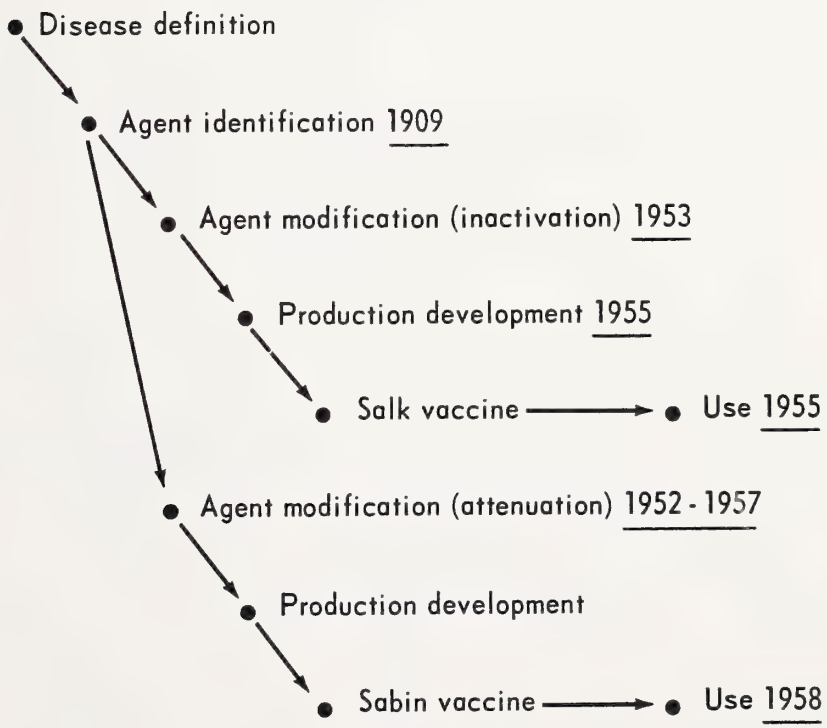


Figure 3

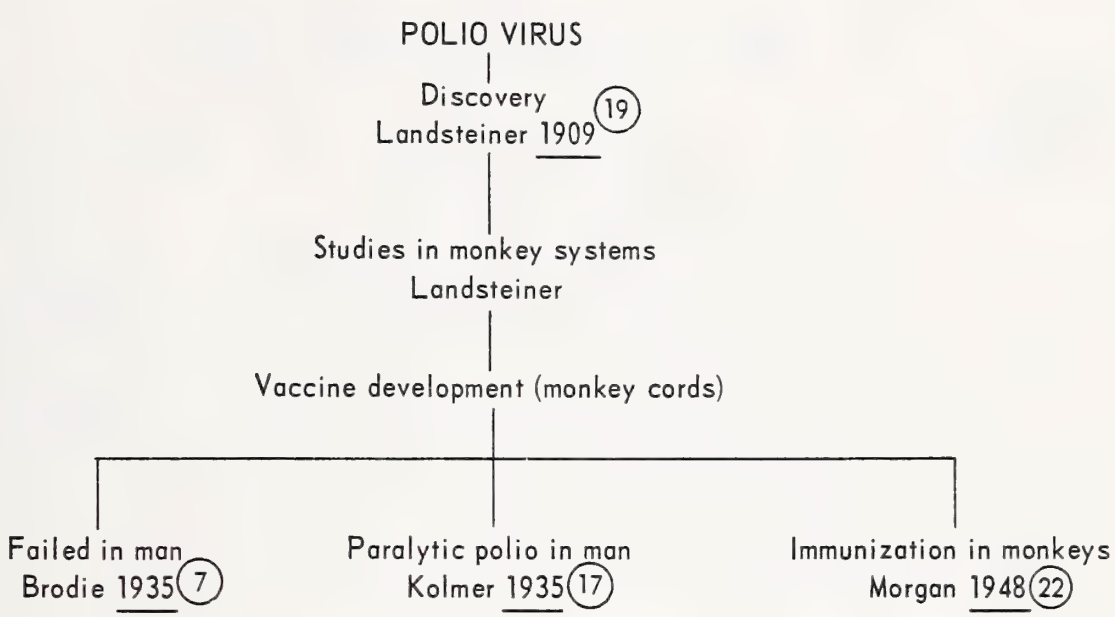


Figure 4

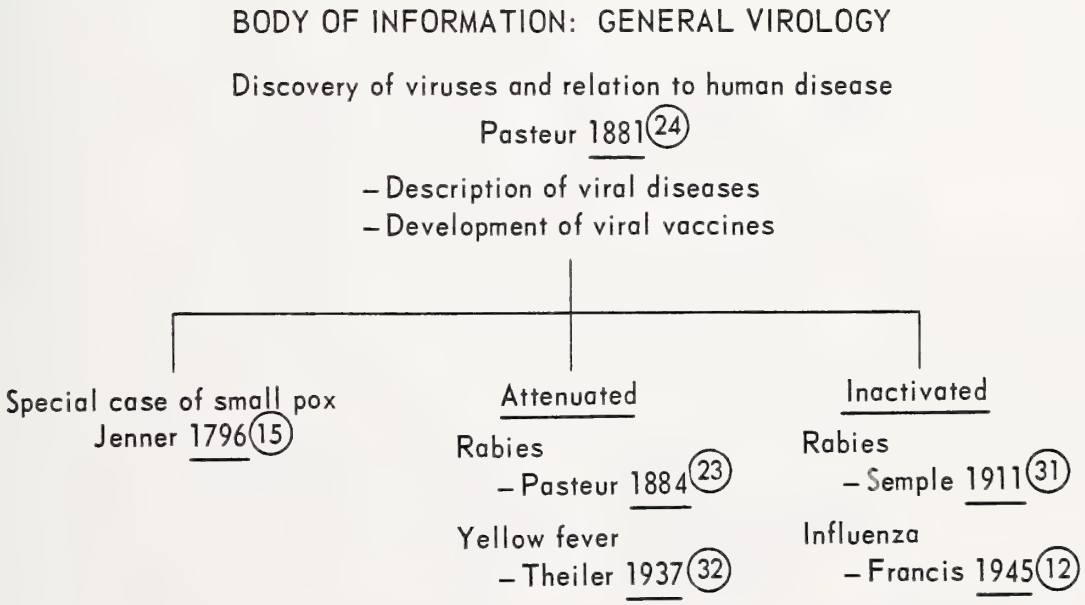


Figure 5

BODY OF INFORMATION: TISSUE CULTURE

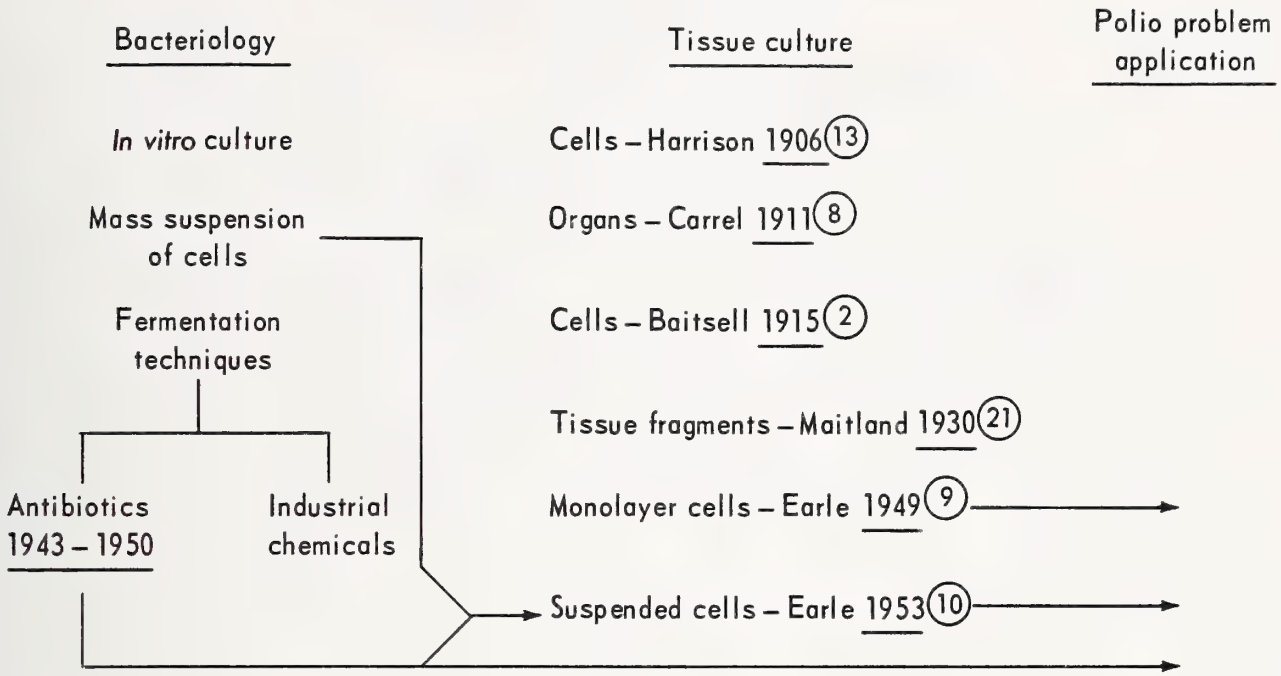


Figure 6

BODY OF INFORMATION: POLIOMYELITIS

Pathogenesis	Human carriers – Kling 1912 (16)
	Nonparalytic infections – Paul 1932 (26)
	G.I. entry – Sabin 1941 (28)
	Viremic phase – Bodian 1952 (3)
	Lifelong immunity – Paul 1950 (25)
Polio virology	New animal models
	– rodents – Armstrong 1939 (1)
	– chimps – Howe 1941 (14)
	Three strains of virus – Bodian 1949 (5)
Tissue culture	Practical virology – Enders 1949 (11)

Figure 7

POLIO VACCINE DEVELOPMENT

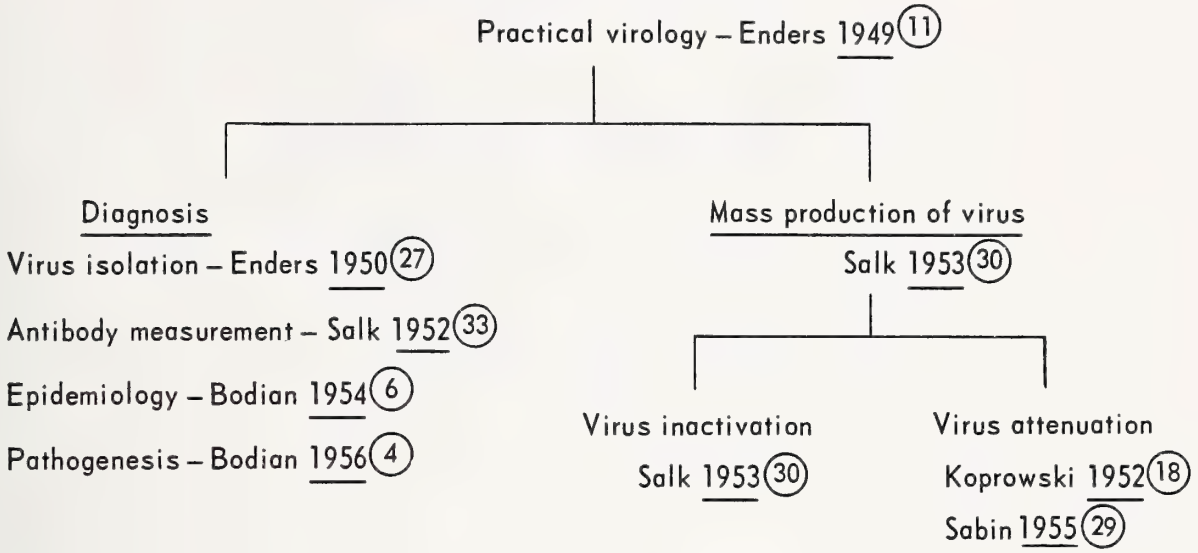


Figure 8

POLIO VACCINE DEVELOPMENT

Phase I

- Immunological approach practical 1933
- Present knowledge inadequate 1936
- Physical barriers (nasal) impractical 1940
- Scientific base inadequate 1940 – 1945

Figure 9

POLIO VACCINE DEVELOPMENT

Phase II

- Saturation support for virology indicated 1946
- Polio caused by three agents 1949
- Disease definition completed 1952
- Tissue culture provides practical means 1949
  - for disease diagnosis and study
  - for virus production

Figure 10

POLIO VACCINE DEVELOPMENT

Phase III

- Vaccine development now possible 1950
- Undirected, saturated support to continue
- Preferred technique – inactivation
- An effective vaccine available 1953
  - field trial 1954
  - general use 1955

  Problem – inadequate science base
- A better vaccine thru attenuation
  - field trial
  - general use

  Problem – lack of U.S. controlled studies

BIBLIOGRAPHY

NOTE: In supporting the thesis, reference is made to certain key observations, and a comprehensive documentation has not been attempted. The reader who wishes to pursue the subject in detail may find this bibliography a helpful starting point.

1. Armstrong, C. The experimental transmission of poliomyelitis to the Eastern cotton rat. Pub. Health Rep. 54:1719, 1939.

2. Baitzell, G. A. The origin and structure of a fibrous tissue which appears in living cultures of adult frog tissues. J. Exp. Med. 21:455, 1915.

3. Bodian, D. A reconsideration of the pathogenesis of poliomyelitis. Am. J. Hyg. 55:414, 1952.

4. Bodian, D. Poliovirus in chimpanzee tissues after virus feeding. Am. J. Hyg. 64:181, 1956.

5. Bodian, D., Morgan, I. M., and Howe, H. A. Differentiation of types of poliomyelitis, III. The grouping of fourteen strains into three basic immunological types. Am. J. Hyg. 49:234, 1949.

6. Bodian, D., and Paffenbarger, R. S., Jr. Poliomyelitis infection in households; frequency of viremia and specific antibody response. Am. J. Hyg. 60:83, 1954.

7. Brodie, M., and Park, W. H. Active immunization against poliomyelitis.

8. Carrel, A. Rejuvenation of cultures of tissue. J.A.M.A. 57:1611, 1911. J.A.M.A. 105:1089, 1935.

9. Earle, W. R., Sanford, K. K., Likely, G. D., and Schilling, F. L. Certain recently developed tissue culture procedures utilizing perforated cellophane as a culture substrate. Anat. Rec. 103:443, 1949.

10. Earle, W. R., Schilling, E. L., Bryant, J. C. and Evans, V. J. The growth of pure strain L cells in fluid-suspension cultures. J. Natl. Cancer Inst. 14:1159, 1953.

11. Enders, J. F., Weller, T. H., and Robbins, F. C. Cultivation of the Lansing strain of poliomyelitis virus in cultures of various human embryonic tissues. Science 109:85, 1949.

12. Francis, T. Jr. The development of the 1943 vaccination study of the Commission on Influenza. Am. J. Hyg. 42:1, 1945.

13. Harrison, R. G. Observations on the living developing nerve fiber. Anat. Rec. 1:116, 1906.

14. Howe, H. A., and Bodian, D. Poliomyelitis in the chimpanzee: a clinical-pathological study. Bull. Johns Hopkins Hosp. 69:149, 1941.

15. Jenner, E. An inquiry into the causes and effects of the variolae vaccinae. (London, 1798).

16. Kling, C. Cause of infantile paralysis. Allm. svenska lak-tidning. 9:1089, 1912.

17. Kolmer, J. A. Susceptibility and immunity in relation to vaccination in acute anterior poliomyelitis. J.A.M.A. 105:1956, 1935.

18. Koprowski, H., Jervis, G. A., and Norton, T. W. Immune responses in human volunteers upon oral administration of a rodent-adapted strain of poliomyelitis virus. Am. J. Hyg. 55:108, 1952.

19. Landsteiner, K., and Popper, E. Transmission of acute poliomyelitis to monkeys. Z. Immun. Forsch. 2:377, 1909.

20. Leake, J. P. Poliomyelitis following vaccination against this disease. J.A.M.A. 105:2152, 1935.

21. Maitland, H. B., and Laing, A. W. Experiments on the cultivation of vaccinia virus. Brit. J. Exper. Path. 11:119, 1930.

22. Morgan, I. M. Immunization of monkeys with formalin-inactivated poliomyelitis viruses. Am. J. Hyg. 48:394, 1948.

23. Pasteur, L., Chamberland, and Roux. New communication on rabies. C. R. Acad. Sci. 98:457, 1884.

24. Pasteur, L., Chamberland, Roux, and Thuillier. On rabies. C. R. Acad. Sci. 98:457, 1881.

25. Paul, J. R. and Riordan, J. R. Observations on serological epidemiology. Antibodies to the Lansing strain of poliomyelitis virus in sera from Alaskan eskimos. Am. J. Hyg. 52:202, 1950.

26. Paul, J. R., Salinger, R., and Trask, J. D. "Abortive" poliomyelitis. J.A.M.A. 98:2262, 1932.

27. Robbins, F. C., and Enders, J. R. Tissue culture techniques in study of animal viruses. Am. J. Med. Sci. 220:316, 1950.

28. Sabin, A. B. Etiology of poliomyelitis, J.A.M.A. 117:267, 1941.

29. Sabin, A. B. Characteristics and genetic potentialities of experimentally produced and naturally occurring variants of poliomyelitis virus. Ann. N. Y. Acad. Sci. 61:924, 1955.

30. Salk, J. E. Principles of immunization as applied to poliomyelitis and influenza. Am. J. Pub. Health 43:1384, 1953.

31. Semple, D. Phenol inactivated rabies vaccines. Sci. Mem. Med. Sanit. Dept. India #44, 1911.

32. Theiler, M., and Smith, H. H. The use of yellow fever virus modified by in vitro cultivation for human immunization. J. Exp. Med. 65: 787, 1937.

33. Youngner, J. S., Ward, E. N., and Salk, J. E. Studies on poliomyelitis viruses in cultures of monkey testicular tissue. Am. J. Hyg. 55:301, 1952.

TAB 82

#435

MEDICINE, BIOMEDICAL SCIENCES, AND ENGINEERING*

James A. Shannon, M. D.**

This talk provides an unusual opportunity for one with a biological background to discuss some common problems with representatives of the fields of engineering and the underpinning physical sciences. The problems that relate to the interaction of engineering and the biological sciences have been a constant preoccupation of mine for the past several years.

During the past decade or so, we have seen the broad incorporation of much applied math and physical science into the investigative fields of biology and medicine, to the striking advantage of the biological sciences. This has taken place within a common forum, the university, which contains the major centers of excellence of science in both general areas.

During recent years, there has been a growing appreciation that comparable rapport should take place between engineering, broadly defined, and biology, broadly defined. My presence at your meetings indicates your conviction as well as my own that this is so. A rapport has indeed evolved in several important areas, but not as a general phenomenon. This, I believe, is the result of several discrete factors:

---

*Presented at the Third Annual Meeting of the American Institute of Aeronautics and Astronautics, Boston, December 1, 1966.

**Director, National Institutes of Health, Public Health Service, U.S. Department of Health, Education, and Welfare.

--the continued imprecise nature of knowledge in biology

--the quite recent emergence of opportunities for goal-oriented research of interest to the engineering professions

--the location of excellence in biology at the university, and the location of engineering strengths in industry

--the lack of suitable coupling mechanisms that facilitate cooperation across the interface separating biological and engineering activities.

There has indeed been good and increasing cooperation between the two in several fields such as instrument development to satisfy specific needs, and the application of computer science and technology to problems of biology and medicine.

There has not been effective cooperation in many other areas--more particularly, those that involve the highly sophisticated techniques dealing with planned, concurrent and sequentially operated activities directed toward definable goals. These are skills that the many members of your organization bring to the problems of the aeronautics and astronautics industry. Such skills can indeed find expression in many complex problems of health care, but not, I believe, in too many complex problems in biomedicine, at the present stage of our knowledge of biological systems.

For a very long time it has been clear that a systems approach is badly needed in the prosaic but socially and economically important fields of

--hospital management, including the gamut of functions which extend from business management to patient management

--hospital systems, including the mix of general and specialized services

--systems that cover the totality of community-based delivery of medical services

--and, generally speaking, the useful handling of the information.

I am glad to say that at least a beginning has been made in each of these areas, the state of the engineering arts being quite capable of immediate contributions.

A systems approach is also needed in a number of very attractive fields of somewhat greater complexity. The first I would touch on is the automation of complex instrumentation. Here, I do not have in mind the next generation of automated mass spectrometers and similarly complex measuring devices for the research laboratory. These are needed, but I believe mechanisms for their development are available. Rather, I have in mind the automation of simple but multiple chemical, cellular, and serological tests that provide the hard data upon which diagnosis and assessment of medical progress is based.

As these clinical laboratory procedures become more numerous and more complicated, it is becoming increasingly difficult for a hospital to maintain the professional staff with all the skills needed to acquire

valid and necessary information for a number of clinical judgments. It seems likely that progress in this field will ultimately require central facilities serving a number of hospitals that as a result of volume in the aggregate, can support proficient technologists. Such a situation will or should provide the incentive for development of broad and sophisticated automated systems.

The second field is that of automation for diagnostic and therapeutic purposes of the many low-frequency outputs of various clinically significant physiological systems. Here, in addition to the conventional EKG and ECG, which too many people are concerned with, I have in mind as examples the development of a capability for handling the total electrical activity of the heart by processing techniques which will convert such electrical measurements to simple statements of electrical status that can be understood by the clinician. Also, I have in mind the development of suitable transducers and data-processing devices that can continuously or repetitively portray such functions as cardiac output or blood flow in specific parts of the circulation.

These are mentioned as examples of current possibilities and needs in bioengineering, but not by way of exploring fields in any depth.

So far we have dealt with relatively simple biological systems and with problems that can be rather precisely defined by the biologist to the engineer. I should like to move to somewhat more complex fields, where one begins to find limitations in this matter of communication.

Here too, we move from a largely developmental field to one of fundamental research. To work on problems of this nature, the engineer must acquire some definitive knowledge and understanding of the biological processes involved, and the biologist, some insight into relevant engineering capabilities.

I am thinking here of very complex problems involving information, communication, and control in biological systems. Study of these will involve the application and extension, for example, of information theory to biological systems. The field includes human control mechanisms, the application of mathematical methods for the analysis of nonlinear systems, and model building with computer simulation of biological control systems. In a very broad sense it can encompass such diverse program areas as machine linguistics on the one hand and the complex operations of the central nervous system on the other.

These are examples of quite advanced areas of scientific inquiry requiring the most sophisticated and, indeed, most challenging efforts of the engineer in biology.

We see, then, that the interface across which the engineer-scientist and the biologist can interact is a broad one encompassing some objectives that are quite feasible today and of great fundamental importance, and others of such complexity that they will not yield to any but the most brilliant minds that are available. But all this is fairly abstract--that is to say, lacking in the sense of the practical,

which I am told is one of the engineer's identifying characteristics. With this in mind, let me touch on two areas that highlight the biological complexity common to many problems of interest to the engineer and the biologist.

The two fields I refer to are not only attractive to the biologist and the engineer, but also the lay science writer. They concern the general problem of organ replacement, specifically the heart and the kidney. The newspapers as well as the scientific journals have kept pace with our joint feeble efforts to replace the heart with a mechanical device or to supplement the kidney, the lung, or the liver with functions provided by physical machines.

Perhaps the most striking of these potential "tours de force" is the development of an artificial heart. This is an interesting subject for initial consideration.

During the 1940's and '50's, pump aerator devices were developed by engineers, physiologists and surgeons that can take over the combined functions of the heart and lungs for limited periods of time. This advance, together with better diagnosis and support procedures and more advanced surgical skills, is the basis for open-heart surgery as we know it today. Essential to the development were a variety of engineering devices that are required to monitor the physiological status of the patient during and after the surgical procedures and others that can produce relative quiescence of the heart during

manipulation, can interrupt arrhythmias, and can reinstitute a regular rhythm at the option of the surgeon.

Thus it was clearly apparent to many physicians and surgeons by the late 1950's that it should be possible to provide temporary or permanent assist or replacement devices to supplement or replace an inadequate myocardium (heart) if this was judged desirable. Viewed from the standpoint of the engineer, this is a complex but not an impossible undertaking, since

--the pressure and flow relationship for both the pulmonary and systemic circulations could be defined with fair precision

--the variations of these in response to stress could be precisely established

--the energy requirements were definable

--there was some information on which to base a further study of materials suitable for implantation.

With such a background, and with a sense of urgency because of the breadth of the medical problem, the National Advisory Heart Council requested the National Heart Institute in 1964 to explore the feasibility of establishing a broad program, developmental in nature, aimed at acquiring a mechanical device that might serve as a replacement for an inadequate heart. The Council, appreciating the complexity of the problem, recommended applying the techniques of the systems approach to the development of components as well as their incorporation into a system.

Appropriate individuals were brought together to consider the problem. They advised a "go-for-broke" approach aimed at developing a mechanical replacement of the heart, with suitable servo control mechanisms to provide adequately for various levels of physiological effort. The device was to be implanted, together with its energy supply and control mechanisms. And there were to be spin-offs in the form of a series of assist devices for emergency, temporary, or permanent assistance to cardiac function.

Medically, it was considered that if such devices were available they might be utilized in a variety of clinical conditions, but would be most useful in the case of myocardial infarction[1] and the resulting myocardial insufficiency. However, it was also clearly apparent on dispassionate examination that even if such devices were available, the clinician does not have the information upon which to base judgments on

--when one of the devices is needed or

--which of them could best satisfy the situation at hand.

In other words, our understanding of the consequences of myocardial infarction is inadequate to permit a decision on the likely further course of an individual case at the point in time when a definitive decision to use or not to use such a device must be made. It is clearly apparent that the majority of patients with infarction who die, do so in the first 48 hours--but this is usually because of cardiac arrest or arrhythmias rather than to myocardial insufficiency. It is

1. The commonly called coronary attack.

also clear that even in the early critical period of the first 7 to 10 days, one cannot predict what the eventual cardiac disability will be--whether maximal benefit would be obtained from a left ventricular assist device or whether total replacement would be required to provide for adequate cardiac function, or whether neither would be advantageous.

With such problems in mind, however, the simple fact of the matter is this: that even if devices were available which had solved the problems of the living-nonliving interface, looking inward to the blood and outward to the containing tissue, and the problems of pumps, their energizers and their physiological control--if all these problems were solved, a critical determination to utilize a specific device could still not be made with confidence. But with the problems more precisely defined in a limited series of patients fitted with left ventricular assist devices because of manifest inadequacy of the myocardium, a systematic program can now be developed.

The first stage of the program will relate to a purposeful clarification of the course of myocardial insufficiency, particularly that resulting from infarction and the utility of support devices of a relatively simple nature. These studies will define in broad outline

--the course of the myocardial infarction in a statistically satisfactory manner, with measurements that will hopefully provide for more adequate prediction of the likelihood of myocardial insufficience and its extent

--the use and characteristics of effective left ventricular assist devices in a limited number of highly sophisticated settings, so as to define their capability--first, when used in a temporary fashion and, later, in a permanent setting if necessary.

Such studies should define not only the desirable characteristics of assist devices and the patients who will benefit by their placement, but also the residuum within the universe of cardiac damage that may require more extensive support. Concurrent studies on materiel, power sources, pumps and control mechanisms to provide mobility can also be planned. In relation to time, I should guess that this phase of the study most certainly will require 3 to 5 years of systematic work before further judgments of program development can be considered.

In another type of organ replacement, one finds somewhat similar problems. I have in mind the development of simplified forms of hemodialysis[2]. Here again, the biological problems are far more complex than those that relate to engineering development.

At present there is no doubt that hemodialysis is a lifesaving measure which can sustain patients with complete chronic renal failure in almost normal activity for very long periods of time. The equipment now in use is not perfect, and there are many other major problems--for example, problems of infection at the transcutaneous point of

2. A technique that "washes out" metabolites that accumulate in the blood when the kidneys are not functioning.

entrance into the vascular system and problems of vessel thrombosis.

Actually, if all the medical problems were solved, we still would not have a technique that is economically feasible for use in all cases of renal insufficiency. The current cost per patient per year is about $10,000, and the complex nature of the setting in which the procedure can be employed safely precludes--at least to my mind--its replication in the present state for general availability.

It is because of these facts that we are undertaking a developmental program to simplify both the device and the circumstances of its use. The extent to which this can be accomplished is not yet clear, except that substantial simplication is feasible.

Meanwhile, dialysis is an essential research tool that can lead to a definition of the specific biological factors that precipitate disability and death in chronic renal insufficiency. Clarification of these factors will greatly modify the next generation of devices. This objective has been brought closer by the recent demonstration of the effectiveness of diets limited to the minimal amounts of essential amino acids as the total source of nitrogen. Hopefully, this will permit definitive study of the origin and nature of the toxic substances that result in uremia[3] and the consequent development of more specific methods than general dialysis as a means for their removal.

Work in other fields relating to a clarification of the role of tissue antigens in the immunological rejection of organ transplants

3. The terminal stage of kidney failure.

will lead to more general use of renal transplants, thus lessening the need to resort to hemodialysis. This, though, will not be the final answer to the problem of chronic renal insufficiency, since the condition largely results from two major diseases, pyelonephritis[4] or glomerulonephritis[5]. The latter disease tends to attack the fresh renal transplant, destroying the new organ even as it had destroyed the original organ.

So one is driven back to the need define the cause of glomerulonephritis; to determine the factors concerned with its progression; and hopefully, with such information, to prevent or modify the course of the disease. Here too there is progress to report in the recent development of an animal model system, occurring in a strain of hybrid mice, which for the first time offers substantial opportunities for systematic study of the disease in the laboratory.

These two developments--the artificial heart and the artificial kidney--illustrate the complex biological setting in which many practical considerations of an engineering nature are found. They were cited to illuminate the problem, not to discourage the engineer.

I am convinced that biology and engineering are now developing--indeed have developed--a very broad interface; that this is producing close and productive contact between diverse biologists and engineers;

---

4. Repetitive infection of kidney substances, ultimately involving loss of kidney function.

5. A chronic disease characterized by acute phases typically leading to complete loss of kidney function.

and that proper exploitation through suitable coupling mechanisms will have profound effects on

--our understanding of disease

--our ability to modify the consequences of disease

--our understanding of many life processes and their control

--our capacity for more precise diagnosis and treatment

--our ability to manage our hospitals, and finally

--our ability to develop more rational systems of patient care.

TAB 83

STATEMENT BY JAMES A. SHANNON, M.D.*

Before Senator Muskie's Subcommittee on Intergovernmental Relations, Senate Committee on Government Operations, at hearings on S. Res. 298, December 15, 1966

Mr. Chairman and members of the Committee:

I am pleased at this opportunity to participate in a discussion of Senate Resolution 298 providing for the establishment of a Select Committee of the Senate on Technology and the Human Environment. My pleasure derives both from my personal conviction that the problems surrounding the impact of technological change upon the human environment are among the most urgent and formidable of contemporary issues, as well as from my official and functional concern with the health aspects of these matters as head of a governmental medical research organization which encompasses a program in the environmental health sciences.

The responsibilities of the proposed Select Committee are both important and broad, and I regret that time has not permitted me to prepare a more definitive statement than I have before me. It has seemed to our staff at the National Institutes of Health that our present purpose will best be served if the time available is utilized to outline the general nature of the problems to which the Committee must address itself--more particularly, as these relate to health. We would be glad at some future date, if it pleases the Committee, to pursue certain of these problems in greater depth.

In order to see clearly the relationship of health to technology and the human environment, it is necessary to have a general concept, or frame of reference, concerning the impact of science and technology on man's life and interaction. I should like to begin by sketching such a frame of reference, with a view to clarifying the values that might be derived from the proposed Select Committee on Technology and the Human Environment in terms of its potential relevance and contribution to health matters.

Technological changes began to have a significant impact on man's life during the second half of the past century. One could trace earlier influences, such as concern for city planning and civil engineering characterizing the Roman epoch; the development of firearms from about the Fifteenth Century, providing an enormous advantage

*Director, National Institutes of Health, Public Health Service, U.S. Department of Health, Education, and Welfare

to technologically advanced societies; or the advent of steampower initiating the Industrial Revolution in the Eighteenth Century. But the great impact of technology on the human condition, bringing changes that might be truly termed a Technological Revolution, has only been felt in America since the time of our Civil War. Thus, it is only for a mere century out of the total span of Western civilization that the forces generating the profound changes that concern us today have been at work.

In summary terms, the effects of this continuing Technological Revolution might be viewed as falling into three major classes:

First, there are the beneficial consequences. We are all witnesses to the vast extension of man's capacity to alter nature to his advantage, yielding goods, reducing toil, accelerating transportation and communication, easing pain, providing recreation, and increasing the lifespan of the species. The benefits of technological change are too numerous and profound even to summarize in any brief meaningful way, but I shall return shortly to the health aspects of this change.

Second, there are the deleterious effects of man's life and environment. Our society cannot escape the issues created by blighted and polluted landscapes, economic conflicts, the physical and psychological hazards of present-day working conditions, dangerous environmental contaminants, and complexities of human interaction and social flux. We are only beginning to encounter the social and behavioral problems of rapidly changing modern life in an era dominated by technology and its consequences. One must also include in this context the modern instruments of warfare, at least in the sense that they constitute a menace to all mankind.

The third major category of effects might be described as the reactive and corrective. This category encompasses the continuing struggle to generate social and governmental actions and institutions that can exert a modifying force over environmental changes which in the past have been largely random, uncontrolled, and unplanned. Legislative changes began in America about the turn of this century, during Teddy Roosevelt's administration, and have continued to the present day. It seems abundantly evident, however, that our progress in developing mechanisms for coping with the effects of the Technological Revolution have never been equal to the scope and diversity of the changes being generated. This is a problem that I believe demands the best thought of the Nation and the wisest exercise of our democratic forms of government.

I should like now to turn specifically to the health scene. The basic strategy of the attack upon disability and disease is the inhibition of hostile forces and the creation of a set of circumstances

engendering optimum well-being. The classic objective of this approach is prevention of disease by determination of the specific causes through an understanding of the underlying etiology and pathogenesis.

Such knowledge has nurtured the remedial, restorative, and preventive achievements of recent history, with emphasis on conditions identified prominently in morbidity and mortality data. This traditional pattern of action has already begun to change significantly, shifting toward concern with broad continuing needs rather than specific emergent problems, and to take up a positive approach to human ecology.

The success of the classic mode of attack on most infectious diseases is well established. Through major advances in sanitary science, vector control, chemotherapy, and immunology, decisive victories have been won over these disease problems. With the notable exception of the viruses, basic comprehension of the causality of overt infectious disease has progressed to the point where techniques for treatment and prevention seem clearly capable of eventual control of the deadlier pathogens.

In recent years the major concentration of energies in the biomedical sciences has been on the broad grouping of diseases which stem from some biological malfunction or inadequacy or peculiar susceptibility. This area comprises the chronic and degenerative conditions, such as the various arthritides and chronic renal diseases, the diseases of metabolic or genetic origin, and broad areas of neurologic and mental disorders. The major causes of death and disability are included in this grouping. Despite the progress to date, the absence of basic understanding of the etiology and epidemiology of such major problems as cancer, cardiovascular and renal disease, and mental illness compels elaboration of empirical methods to temporize with these diseases as best we can. Really effective and rational methods await the further advance of basic knowledge.

Despite the magnitude, severity, and urgency of the gamut of health problems presented by these diseases, and the enormous distance yet to be traversed in reducing their toll of premature death, disability, and suffering, there are intrinsic limits to progress in these areas. Here, in our health efforts, we come face to face, of course, with the inexorable process of aging and the immutable fact of death as essential attributes of the biological process. Therefore, despite the intensity of the engagement on this front, now and for the future, our concern with the process of dying has an absolute limit. The ultimate function of our health efforts lies not in this melancholy consummation, but in concern with the process of living.

Thus, at the moment of our great victories over many infectious diseases, and in the midst of our full engagement with the major diseases and conditions of basic biologic origins, we are now confronted with the realization that the most formidable and pervasive challenges to the well-being of man lie in the infinite complexity of this highly differentiated organism and in the hazards man has created for himself in the products, processes, and living patterns of his increasingly technological world.

It seems not unlikely that the very effective immune mechanisms essential for man's handling of invasive agents of a bacterial or viral nature can create circumstances of a biological nature that produce serious disease. I have in mind the current concept that a number of serious diseases, such as rheumatoid arthritis, are the result of so-called autoimmune processes--pathologic reactions to endogenous agents or conditions.

As to environmental hazards:

. . .Accidents now rate as the major cause of death from the first year of life to age 36.

. . .The increasing dispersal of toxic chemicals and other substances of largely unassessed biological effect in air, water, and food engenders mounting public apprehension.

. . .The expanding use and diversity of sources of ionizing radiation threatens widespread biological damage.

. . .The complexity of industrial development and activity, and urban living with consequent growth in noise, strain, speed, tension, and social instability, pose grave psychogenic potentials.

These and similar environmental hazards have been brought about by the transition of man from an environment dominated by nature to an environment dominated by technology. They will long present a major potential for harm to individual and community well-being. Many of these problems will be of such complex nature and involve so many interacting variables that the demonstration of health consequences to man in terms of specific pathology related to specific causality will require long periods of observation and be increasingly difficult, if possible at all.

For several years, problems associated with the environmental assault on health have been a matter of intense interest and activity on the part of the Public Health Service. The research aspects of

this concern are now focused in the Division of Environmental Health Sciences, newly established in the National Institutes of Health as a consequence of the reorganization of the Public Health Service now being implemented.

The center of scientific interest of the new Division will be the sources and the adventitious effects of environmental substances on biological systems and the effects of those on human health and well-being. Inquiries will encompass the epidemiology of chemical and biological hazards, their distribution and transmission, and their short- and long-term effects on both animals and man.

The objectives will be to assess the actual and potential risks of exposure to environmental hazards, and with an understanding of the biological systems involved, to furnish a science base upon which it is possible to determine tolerances as a guide to control policies, and to advance control technology.

The vigorous pursuit of knowledge in these areas, we are sure, will have the rapport of the Congress, even as they now support a broad program of research into the many diseases that form the basis of the categorical structure of NIH. But these diseases, together with the consequences of an increasingly hostile environment, will not easily yield the necessary information required for their control.

In the meantime the needs associated with the provision of health and medical care services to cope with illness and disability in a growing and changing population present complex and pressing problems. This Congress has been involved at the very center of these issues. They are represented by the problems of financing medical care, specifically for the aged and for the indigent; our national concern with the availability and accessibility of adequate diagnostic, hospital, nursing home, and rehabilitative services and facilities generally; and our seemingly endless expansion of the need for trained health manpower. It is here that the advances deriving from the science and technology of medicine encounter the difficult and controversial problems that surround the economics, sociology, and patterns of relationship in the delivery of health services.

There is a growing awareness of the differential between the quality and scope of the medical services available in the centers of scientific and academic medicine and those available to the bulk of the Nation's population. We are directing increasing effort to diminish this differential; but as I have noted, these problems lie essentially beyond the domain of science and technology; and the issues in this area have their roots in traditional patterns of action, in the set of

values emphasized by our society, and in the way those values can be reflected in social, political, and governmental action.

This I believe brings us to the key matter relating to the function and role of the proposed Select Committee on Technology and the Human Environment. To make this point, I should like now to turn briefly to the broader scene.

It is possible to view the development of this Nation in terms of four broad periods:

1. The period of initial economic and political development, when the concern of the Federal Government was to provide for our commerce and trade, the exploitation of the frontier, the strengthening of the Nation's agriculture, and latterly, the cultivation of a strong industrial base. This period occupied the first century of the Republic's existence, in the midst of which a national struggle over the basic character of the Federal Government and the Union was resolved.

2. The second period culminated in the events which took place during the 1930's to provide broadly for the social and economic security of the Nation as a whole. This period was characterized by a shift in the view of the function of the Federal Government from essentially a passive to an active force in advancing social conditions and welfare.

3. More recently, concern has centered upon extending the general social and economic benefits of our national growth to the disadvantaged components of our society as represented by the aged, the poor, and the minorities bearing the burden of discrimination.

4. In the midst of these current concerns, the advance of science and technology has increased the national consciousness of the vital character of the human resources of the Nation and the value of its intellectual potential. These are the real measures of national prowess and capacity in this contemporary world. The latter realization has led the Nation to cultivate these precious resources through the expansion of educational opportunities and facilities and through the general concern for the broad conditions of our national life.

Thus, as our ability to change the environment of man and to affect the circumstances of human life and society grows more powerful and

extensive, we must now direct our concern to controlling and directing these developments toward the highest humanitarian values and goals. The wholly pragmatic and opportunistic economic and industrial considerations which have for so long exerted the dominant influences upon man's environmental development and his biological and social adaptation cannot be allowed to continue.

Thus the challenge to our society is twofold:

> . . .to diminish the untoward effects that derive from our technological revolution. This is urgent because the prospective hazards associated with the advance of technology are growing more complex, more far-reaching, and more difficult to control or reverse.
>
> . . .to assure that the extension of this technology enhances the quality of life. We are gaining an enormous potential to shape the conditions of life and the quality of our environment from which we must secure the maximum benefit.

As I have noted before, these challenges call for the wisest exercise of our governmental processes in influencing the course of future technological development. Yet as we in the Federal Government --both in Congress and in the Executive Branch--have addressed ourselves to these challenges, we have tended to identify and to attempt to meet individual issues, often giving each a distinctive organizational setting. The holistic approach has been missing; and the result has been widespread fragmentation of responsibilities. This way of doing business has diffused the Federal discharge of responsibilities, and has tended to confuse State and local relationships to these Federal efforts. The urgency of action to correct these problems is, I believe, among our paramount national concerns.

These are the matters which have inclined me favorably toward the concept of the Select Committee on Technology and the Human Environment. I think we must make every effort to assure intelligent action in dealing with these problems.

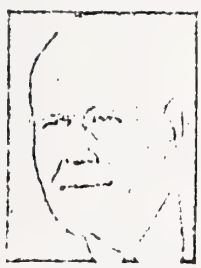
Clearly, a matter with which this Committee must concern itself is how to achieve the most effective relationships among the respective levels of national action on these problems. How can efforts at the Federal, State, and local levels complement and reinforce one another in dealing with these broad needs? This seems to me to be a vital consideration, particularly as it relates to governmental functions.

It is through the processes of Government that we establish our national goals, exert our sense of human values, and secure the strength and resources to these ends. I believe that Senate Resolution 298, if enacted, would be a helpful and useful step in assisting the Congress in its engagement with these vital matters.

TAB 84

EDITORIALS

Irvine H. Page, M.D.
Editor



Walter C. Alvarez, M.D.
Editor Emeritus

James Shannon—a vignette of a great "bureaucrat"

Jim Shannon, the director of the National Institutes of Health (NIH), is a medical and scientific phenomenon, one vitally important to all of us. It is he who has largely shaped the current form of both the intra- and extramural program of these powerful Washington institutions. To him we owe some of the most important overall policy changes in medical education and research.

And now within two years he is to retire. His loss, in my opinion, will be so great that each of us must take on some of the responsibility of seeing that a worthy successor is appointed. This is no trivial matter and should not be left to concealed maneuvers so often miscalled "politics." Let me insist that this job and the man in it are our responsibility if only because for good or for evil, this person will be spending vast amounts of our tax money. It is none too soon to consider a replacement. The notion that many should expose their views in open discussion about such an appointment will be considered wholly unorthodox and by many wholly out of order. However, since Washington uses its power to change *our* lives, we had best participate in the decision of who makes the changes.

Why is this one man so important? Chiefly because he is the focal point of the entire program of the National Institutes of Health and their relationship to both the medical public and the federal government. The director of the National Institutes

must understand both the scientific and the administrative aspects of this great complex. It is an almost overwhelming problem to find one man so talented. He must appreciate and sympathize with the problems of research workers, practicing physicians, medical educators, legislators, government financiers, and, finally, the President. They all have a finger in his pie and most of them think they know better than he what to do with it.

He must be able to advise all these persons on the myriad problems that face medicine and science. He must answer with authority questions asked by committees and commissions on topics ranging from *rubella* to accounting practices of universities. And he is held responsible for the slightest slip. This kind of interrogational third degree makes for wonderful reading, especially in the hearings of the Committee on Appropriations, with Representative Fogarty as the grand, and I must say highly perceptive and usually sympathetic, inquisitor. To outsiders, the range of testimony is almost unbelievable. No one, it seems, can really know so much, yet on this testimony depends much of our own fate as reflected in legislation.

There are many such "high-level" functions, but beyond this is the day-in and day-out job of appointing the right men to the right spots in the huge institutions that compose the National Institutes. The director must know medicine and science as well as its people—a "hell of a job."

How has Jim Shannon handled it? He began many years ago by establishing himself as an international authority on the physiology of the kidney as a result of ten years of work at a laboratory bench. He applied the newly developing "clinical pharmacology" to the antimalarial program during the war. Instead of just a screening program because of the urgency of demand, he knew the program would become sterile unless it was associated with fundamental research. Shannon's group showed that the poor performance of quinacrine (Atabrine®) in treatment of malaria was due to the extraordinary capacity of tissues to take up the drug; the plasma levels simply were not high enough to kill the plasmodium. The problem was solved by giving a large initial dose and then small daily doses to maintain the level. He showed early the capacity to direct research without "directing" it. He pointed investigators in the

right direction and then stimulated them to take off. He was not one to duck the responsibility of research by not participating in publication. He put his name on the dotted line like any responsible investigator.

Let me give just one example of the fact that Shannon was not just a man in a plush office. He saw and criticized all manuscripts before they were sent in for publication and laboriously taught doctors and scientists how to use the English language. Youngsters today grow up under a system in which they answer questions by *yes* or *no* or *X* as a measure of their intellectual level. English need only be mumbled, not articulated!

He recognized the fact that a practical problem often is solved only by doing those things considered academic or fundamental—things which recently seem to have been downgraded from on high as being inapplicable to the health problems of our citizens. Then Shannon exhibited that unusual, and to me wholly inexplicable, capacity to pick the right people! The list of those who worked with him during this early period composes a who's who of current competence—i.e., Axelrod, Bang, Berliner, Brodie, Chalmers, Earle, Farber, Greif, Knox, Taggart, Udenfriend, Zubrod.

Shannon gathered more administrative experience while director of the Squibb research program and shortly thereafter became associate director in charge of research of the embryonic National Heart Institute. Immediately he picked very young men in whom he sensed the capacity for great research and set them at their task. He recognized that the NIH could not live in an academic vacuum so he established rapport with the medical and scientific world outside. The door was always open to the sort of intimate talk which keeps a man in touch with what people truly think. He encouraged the establishment of a very active pre- and postdoctoral training program and had the good sense to train people and then turn them out. With the permanent staff at a minimum, deadwood did not accumulate.

He became director of the National Institutes in 1955, and in 1957 he established the Research Associate Program to train young physicians intending to enter research as a career. Including clinical associates, there are currently some 200 such temporary research

workers staying for periods of two to three years at NIH. This is a young program yet its results are already obvious—i.e., James Wyngaarden, professor of medicine at Pennsylvania; Roy Vagelos, professor of biochemistry in St. Louis; and Donald Frederickson, director of the National Heart Institute. What more could you ask?

Shannon in a sense must seem a paradox to those not trained in research. He is not the kind of "director of research" one finds in large corporations concerned as much with power and profits as with the conduct of science. He understands how to create the environment in which research flourishes—a sensitivity that will never be appreciated by many and is only too easy to destroy. Too much or too little of the necessary ingredients can seriously upset the delicate balance and reduce effectiveness. I believe that most people able to create such environments begin as good scientists and later become administrators.

I have said enough to indicate that Jim Shannon is a pretty special sort of man! I have been on the lookout for many years for paragons and have found few, but he is as close as any I have seen. That is why he will be so hard to replace. He has set the requirements too high. A great challenge is there to meet the future incumbent. We must not accept second best or mediocrity as an easy way out or as a political expedient.

This is a problem I wish would go away and not bother us. But I'm afraid letting somebody else worry about it is not good enough. The final decision rests in the hands of Secretary John Gardner and the President, but the selections and recommendations should come from all of us who care for the future of science and medicine.

In many ways the decision is more important than more legislation or more money for research and medicine. The job may be greater than any individual, but it still takes an individual to keep its high ideals alive.

Never forget that it is people who make the environment of medicine and science in which you and I live and work. Each of us in our small way will help to shape that environment if we make it our concern to see that a man of whom Jim Shannon can be proud is selected as his successor.

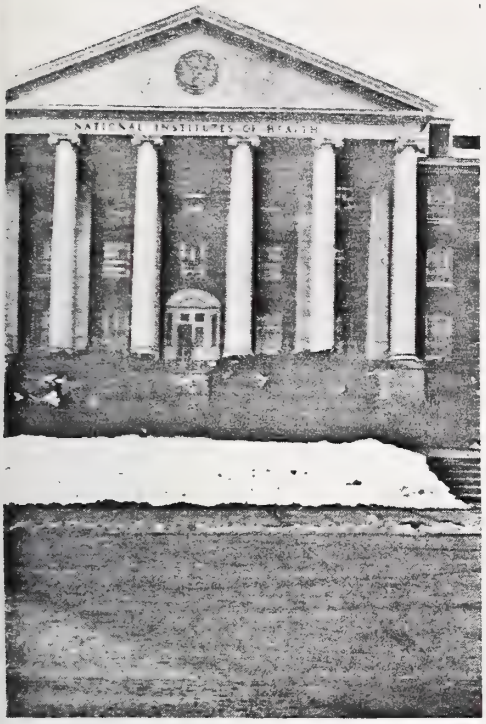
IRVINE H. PAGE

TAB 85

BUSINESS WEEK
December 31, 1966
Fifty cents
A McGraw-Hill publication

General of the war on disease

The top man at the National Institutes of Health holds a key scientific policymaking post. While he allocates vast sums for research, his concern is middle-age killers



Headquarters for the U. S. assault on disease are located in Bethesda, Md.

James A. Shannon, former basketball star, will spend $1.4-billion this year.

RESEARCH

General of the war on disease

The top man at the National Institutes of Health holds a key scientific policymaking post. While he allocates vast sums for research, his concern is middle-age killers

"People have to die sometime. I think it's about time we talk about the quality of living rather than dying," says Dr. James A. Shannon, director of the National Institutes of Health.

A 62-year-old scientist, he administers the biomedical research arm of the U. S. Public Health Service whose purpose is the health and well-being of every U. S. citizen.

Shannon's job is one of the top science policymaking posts in the world, and this year he is administering a budget of $1.4-billion. He runs a 300-acre, $83-millon establishment at Bethesda, Md., 12 mi. northwest of downtown Washington, D. C., and 12,500 people (including 4,020 degree-holding scientists) work for him. NIH also has research facilities in 10 other states and offices in four foreign nations.

The agency allocates 40% of the money spent for medical research in the U. S., including more than one-third of all money made available to institutions of higher education for this purpose. Some 80% of NIH funds go directly to 2,000 colleges, universities and research institutes.

I. Goals

Shannon's job has the ultimate aim of solving problems of disease. He is not too concerned with how much further medical research can extend life expectancy beyond the present 71 years. Aging and death are irrevocable facts of life, he says, "but I am concerned about the social damage that takes place in a family when a father or mother dies in their late 30s or early 40s."

He takes a practical position on how far medical science can and should go to extend life, and his view is tempered with solid grounding. He did his undergraduate work at Holy Cross, where he played basketball and was on the track team; went to New York University School of Medicine (class of '29); won a Ph.D in physiology from NYU; spent 15 years as a practicing doctor-teacher-research scientist; and for three years was in industry as director of the Squibb Institute for Medical Research.

Timetable. Twenty-six years elapsed between the time Shannon finished medical school and his NIH

appointment on Aug. 1, 1955. By then, as one NIH aide sees it, the agency, born in 1945, had completed its childhood. Shannon would direct it through adolescense (1956-1960) and into maturity (1961-1966).

In 1945, NIH was only a small federal laboratory with a budget of a few million dollars. Today it ranks as the leading biomedical research center in the world. At one time it was concerned only with grants to academic institutions; today it is involved with every civilian public-policy science decision and all the problems—from air pollution and aging to heart disease and health care—of living in a society beset by the strains and tensions of a technological revolution.

"At the NIH," says Joseph S. Murtaugh, chief of the Office of Program Planning, "there's no question that Jim is both the chief scientist and director." The chief scientist tag is well earned. Shannon has made fundamental contributions to medical understanding of the physiology of the human kidney.

II. Gains

During the period that Shannon has directed NIH, the explosion in scientific knowledge has rocked the world. It has brought both fantastic medical advancements (from open heart surgery to manipulation of the mind) as well as nagging problems that beg for answers, (who shall live by artificial means, such as mechanical kidneys?).

Today Shannon can look back on almost 12 years of impressive accomplishments.

The NIH budget has increased 17-fold—from $81.2-million in fiscal 1955 to $1.4-billion this year. This money has financed countless research grants for both far-out and practical studies, ranging from how nematodes transmit disease to designs for an artificial heart.

These grants also transcend national boundaries; foreign scientists with worthwhile projects get money, too. One of the three French winners of the Nobel Prize in Medicine last year was a NIH grantee, and in fiscal 1966, NIH spent $16.2-million in foreign grants and awards.

Men and education. NIH's facilities in Bethesda also have served to remove the stigma that many government agencies are just a place to get money. NIH scientists are tops in their fields and many times have pointed the way for others to follow with fundamental research discoveries.

The agency also is deeply involved in medical education, giving 30,000 fellowships and training grants for advanced education, providing long-term support to 65,000 senior research investigators, as well as the funds to build or renovate 17-million sq. ft. of new research space over the past 20 years.

Although Shannon plays down his role in this phenomenal growth, 13 academic institutions have awarded him honorary degrees. He credits the legislative branch of the federal government for seizing the initiative in the early 1950s, when research in the biomedical sciences was ripe for exploitation.

He points to men such as Senator Lister Hill (D-Ala.) and Representative John E. Fogarty (D-R.I.) who became champions of health legislation, as well as Marion B. Folsom, former Health, Education & Welfare Dept. secretary, and Dr. Leonard A. Scheele, surgeon general from 1948 to 1956.

Modesty. While Shannon may be self-effacing, those working with him are lavish with praise. Dr. John F. Sherman, director of NIH's extramural (beyond Bethesda) grants and contracts program, calls him "a rare combination of first-class competence, with an acute political sensitivity of Washington and of the scientific and academic community."

Dr. Stuart M. Sessoms, deputy NIH director, who is described as Shannon's mediator of internal squabbles, compares Shannon's administration to a symphony with Shannon as director. "I might be first violin, but at other times I might not even be in the score," he says.

Shannon pushes his staff, but he also pushes himself. Night phone calls and "think" sessions are common. He usually rises at 6 a.m. and works at home for 2½ hours. His vacation this year was spent at Bethesda. He still came to his office, only not as early; it was his chance to say "no" to some appointments.

Conferences, reports, planning sessions, meetings, speaking engagements, and testifying before Congress occupy his time. In his writings, the overriding theme comes from a spark he received in medical school, "the need for a scientific base for modern medicine."

Definitions. This forms the base of what Shannon calls "fundamental research," a term he prefers to "basic research" (which he calls a National Science Foundation concept of "research for research's sake"). He feels scientists should be pragmatic and work on problems that could have immediate applications with long-range implications as well.

However, he dislikes "crash" pro-

Shannon and three top aides convert paper problems to attack on disease. Joseph S. Murtaugh (far left) writes much of the language, John F. Sherman directs extramural grants programs, and Stuart M. Sessoms is trouble shooter.

grams. Instead, Shannon wants assurance that "we have in the works fundamental research that will provide the knowledge that will later allow accomplishment of a goal."

Last June, Shannon and his nine institute directors—for heart, cancer, mental health, dentistry, arthritis, neurology, allergy, child health, and general medical sciences—met with President Johnson. At that time, Johnson called for concrete results against "killer" diseases, saying that "we must make sure that no life-giving discovery is locked up in the laboratory."

While many scientists, engaged in basic research, felt the President's comments downgraded their work, Shannon took an opposite view. The President, he recalls, merely said "he was interested in science but more interested in results."

III. Targets

To answer the nation's health problems, NIH wants cures, vaccines, and results. To this end, Shannon and his advisers are pushing down many avenues:

- Contract research programs are under way to develop new cancer chemotherapy agents, to understand the virus-leukemia link, and to launch an all-out attack on breast cancer.
- Research is under way to develop effective vaccines for German measles (rubella) and for upper respiratory diseases.
- Contracts have been let for design and development of a less expensive artificial kidney (the cost of treatment now can go as high as $10,000 yearly).

Shannon says that large-scale dialysis machines to filter impurities from the blood would require federal subsidies. However, such devices could be used as a research tool to study toxic substances that cause kidney failure and uremic poisoning, and perhaps lead to a better way to remove toxins than dialysis—a crude but life-saving procedure.

Of the heart. Last month, though, Shannon toned down a "go-for-broke" effort to develop an implantable mechanical heart that had been set in motion by the National Heart Advisory Council. He reasoned that there was a lack of fundamental knowledge of cardiac arrest and arrhythmias (irregular and uncontrolled beating), the primary causes of death in the first 48 hours after a heart attack.

Instead, Shannon sees a five-year program undertaken by selected scientific teams in "myocardial insufficiency study centers" to look into heart-assist devices, which already are under test in Houston and Brooklyn. Shannon wants to explore the capabilities of partial artificial heart assist devices and then expects to have the information to determine whether an artificial heart is needed.

While NIH wages its war against the killer diseases, it moves deeper into certain neglected areas of health problems—neglected because of a lack of researchers and money to do the work: dental diseases, mental retardation, the biology of reproduction and development, aging, and environmental health.

The impetus for some of this work will come from NIH's Institute for Child Health & Human Development and the Environmental Health Sciences Div. Of the latter's research effort, Shannon comments, "We know nothing about the cumulative effect of body levels of pollutants. It's just a vast sea of ignorance."

New field. In a speech in Boston not long ago, Shannon called on the aerospace industry to apply its knowledge of systems development to bio-engineering and try to lick some age-old problems of medicine. He feels that major opportunities exist in computer technology in hospital administration, laboratory processing, and patient information systems. NIH itself is looking more toward the systems development approach to problems of disease. "In a large part," comments an agency planner, "the future source of scientific and technical capability to do the job will be private industry."

Despite much progress in medical research in recent years, Shannon freely acknowledges that a real gap exists between existing knowledge and its application to patients. This is termed by many as "uneven medicine."

Shannon has gripes—as well as ideas—about what can be done.

He thinks it a considerable disgrace that some 1,500 foreign doctors were imported to the U. S. last year. He suggests a new type of health professional, similar to a military medical corpsman, and the mandatory retraining and updating of practicing physicians every five years.

Leadership. A need exists, Shannon says, for "an enlightened medical leadership in the U. S.—a spokesman for the people." Two encouraging trends are a move for a National Academy of Medicine and the increasingly active role being assumed by the American Academy of General Practice.

Meanwhile, "a new social purpose," as one NIH aide describes it, has been assumed by the Regional Medical Programs Div., organized last February. Already it has made 30 planning grants to regional medical groups in the nation, who are exploring ways to coordinate their health services.

Shannon, though, keeps on with his hectic days of formulating biomedical policy that is felt around the world. He faces mandatory retirement in 1968 unless kept on through administrative order. Meanwhile, he and his successor face a number of tough moral and ethical decisions. For instance, how far should medical research extend life?

Shannon generally agrees with some religious leaders who express the view that there is no moral merit to using extraordinary means to prolong life. "In the abstract," he says, "that's the attitude of most people. But it's awfully hard to sell to the family." **End**

TAB 86

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE

Statement by Director, National Institute of Health
Public Health Service

on

1968 Appropriation Estimates

Mr. Chairman and Members of the Committee:

There are twelve separate appropriations in the Bill now before you which, together, provide funds for the programs of the National Institutes of Health. The Directors of the appropriate Institutes and Divisions will provide detail about the specific aims, accomplishments, and needs of the activities supported by each of these appropriations. I should like to speak about their common purposes and their common problems.

Medical research in this country has been, and is, so closely interwoven with the evolution of the NIH support programs that it will be logical--and, I think, of special interest to the new members of this Committee--to outline the development of these programs.

At present, about 40 percent of our total national expenditure for medical research is provided through the NIH appropriations. NIH is by far the largest single source of support for medical research in this country. The character of the programs through which this support is provided--and, incidentally, the manner in which they are administered--plays a substantial role in determining the character and directions of our national medical research effort and, in a very real sense, the quality of life of the American people. I suspect that most Americans agree with the view expressed

some years ago by Professor Will Durant, who popularized philosophy in the 1930's, that "The health of nations is more important than the wealth of nations."

The tremendous strides that have been made during the past twenty years in our understanding of biological processes and in our ability to deal effectively with many disease problems has added immeasurably to our national wealth. This is true if this progress is estimated, however imprecisely, in economic terms--such as added years of individual productivity or the reduction in time lost from work. It is overwhelmingly true if the measure is humanitarian--the suffering that has been eased or prevented, the lessening of permanent disability, the young children and young parents whose lives have been saved, or the increasing number of people who are able to enjoy a vigorous and healthy old age. It is difficult to estimate in a meaningful way the consequences to a child and his family of the avoidance of death or disability due to polio, or the restoration to normal development of the child with complex congenital heart disease, much less the consequences to the family unit of the early detection and care of uterine cancer.

There is in progress a quiet revolution in the practice of medicine as the result of research. The advent of antibiotics has sharply reduced the ravages of unpreventable infections; new techniques for cardiac and other forms of surgery have made it possible to intervene in situations which a decade or two ago were beyond surgical help; drugs for the control

of hypertension have significantly reduced the death rate for heart disease among middle-aged persons; a number of viral vaccines are now in common use.

The American Medical Association recently reported that the introduction of new products has been so dynamic that 7 out of 10 now on the market were not available ten years ago.

New knowledge has greatly changed the approach to the treatment of disease and the attitude of the practicing physician to science. Fifty years ago, few physicians were concerned with scientific investigations--their main professional interest was the application of existing empirical knowledge which changed very little from year to year. Today, while relatively few practicing physicians are themselves engaged in formal research, they are fully aware of the impact of science on their capability to diagnose and treat disease.

But I am less concerned about the progress of the past than I am about the vast problems that still remain to be solved.

Despite the great strides forward in recent years, the biomedical sciences are still in a primitive stage of development. The lack of understanding of underlying causes is still the main barrier to the solution of the major chronic illnesses. Much of biological research must continue to be descriptive and observational and its approach to the solution of problems is still, necessarily, empirical. The unifying and clarifying principles and 'laws' that transformed alchemy into chemistry and natural philosophy into physics have yet to be uncovered in the life sciences. In fact, too

frequently a major step forward brings into view still larger--and more forbidding--areas of unknown territory. For example, the success in combating bacterial infectious diseases laid bare, for all to see, the puzzling roles that seem to be played by viruses in both acute and chronic diseases.

The puzzling and exasperating delays that mark the progress of medical research are due to the large gaps in fundamental knowledge. The history of immunization against infectious diseases provides a typical example. The demonstration that inoculation with infective material from a calf would prevent smallpox took place in 1796 but because it came about without any real understanding of the nature of the disease or of the principle of vaccination, it remained, for nearly a century, simply a technique for preventing a specific disease. Not until Pasteur, Koch and others established the principles of bacteriology in the 1880's, was it possible, in a relatively short time, to control such diseases as diphtheria, tetanus, bubonic plague, and scarlet fever.

Even so, basic knowledge was still insufficient to cope with many other infectious diseases. The existence of 'ultravisible viruses' was deduced in 1898 but many years elapsed before it became possible to isolate them or grow them in a laboratory. Polio, for example, was identified as a virus disease as early as 1909 but it took more than 40 years of scientific effort on many related problems to pave the way for a successful vaccine. Not until the 1950's were knowledge of the disease and techniques for vaccine development adequate to the task. It is, incidentally,

instructive to note that the tissue culture techniques used were the result of research having no direct relation to the polio problem. They were developed partly by scientists studying the characteristics of cancer cells and partly by scientists trying to improve the production technology of the fermentation industry.

We are now in a period of rapid development of viral vaccines that is, in many ways, comparable to the progress that followed Pasteur's discoveries 80 years ago. In addition to the polio vaccines, we now have vaccines for such common viral diseases as yellow fever, influenza and measles and for several less common respiratory diseases. In the foreseeable future, we shall have vaccines for German measles (rubella), mumps, and a number of other respiratory diseases whose causative viruses have been identified.

In other fields, the problems are more difficult. For the chronic diseases with which medical research is now primarily concerned--heart disease, cancer, an array of metabolic and neurological diseases, and congenital defects--the valleys of ignorance are broader than the peaks of knowledge. The exploration of the cause of these diseases or defects--which must precede the development of wholly effective treatment and preventive measures--is most formidable. In fact, many of these diseases probably have no single cause. Rather, the cause of disease and the determinants of the rate of progress in dealing with it are likely to be a combination of chemical and biological, environmental and developmental, and behavioral and sociological factors. A total understanding of these diseases will require research in many disciplines and in many directions.

The change in the pattern of major health problems from the acute infectious to the more complex chronic diseases and the consequent change in the research emphasis is paralleled in the evolution of NIH and its programs.

NIH had its beginnings in 1891 in a small bacteriological laboratory at the Marine Hospital on Staten Island. This was soon transferred to Washington and became the Hygienic Laboratory. For its first 20 years, it was exclusively concerned with communicable diseases which were then the major public health problem and accounted for more than half of all deaths. About the time of World War I, its activities were gradually expanded. The Hygienic Laboratory discovered the cause of pellagra, a dietary deficiency disease then common in the South, it did pioneering work in the study of shock resulting from immunizations, and it formulated the first sound theory about personal sensitivity that helped to lay the foundation for the study of allergies--which were until then unknown and unsuspected.

As the application of the new science of bacteriology progressively brought the major infectious diseases under control, the death rate for these diseases rapidly declined--from 648 per 100,000 population in 1900 to 208 in 1937. By 1950 the rate was down to 65--a tenth of what it had been half a century before. But partly as a result of this success, the other major causes of death and the so-called chronic diseases loomed larger as national health problems. The death rate for malignant neoplasms

rose from 64 in 1900 to 112 in 1937, for cardiovascular and renal diseases from 345 to 455, and many people who would formerly have fallen victim to diphtheria, smallpox or typhoid fever now survived to enjoy an arthritic old age. The first significant step to extend the research activities of NIH to these fresh areas of public concern was the creation of the National Cancer Institute in 1937.

The National Cancer Act of 1937 marks a sharp turning point in the history of NIH. It set the stage for a series of actions which were completely to alter the scope, orientation and mechanisms not only of the NIH programs but of the whole national biomedical research effort. The ultimate effect of the pattern set by the Act was

... to focus attention on specific diseases, or groups of diseases, and to authorize special programs to discover the cause, facilitate the diagnosis, improve the treatment, and, hopefully, devise preventive measures for these diseases;

... to complement the research conducted by NIH itself with grants-in-aid to non-Federal institutions for research projects directed towards the same ends;

... to give NIH responsibility not only for the conduct and support of biomedical research but for research-training through the award of fellowships; and

... to dramatize these disease-oriented programs--and give them greater public visibility--by grouping them in

separate categorical Institutes named for the major diseases entities within their area of responsibility.

The transformation did not, of course, come about all at once. The Cancer Act merely authorized a comprehensive cancer program that included research projects grants and fellowships for this purpose only. Its immediate effect was to create twin Institutes in 1937--the National Institute of Health and the National Cancer Institute--on the new grounds in Bethesda. But the new grant program awarded only 9 research project grants, totaling $91,000, in 1938.

The intervention of World War II slowed the pace of development and--except for budget increases to off-set war-time inflation and to pay for special war-related research projects undertaken by NIH--the situation remained fairly static. In 1945, the two Institutes had appropriations totaling $2.8 million of which $170,000 was for research grants. Total national expenditure on medical research, from all sources, at this time was about $70 million.

The Public Health Service Act of 1944 extended the authority to award research-project and research-training grants to the other programs of NIH and prepared the way for the post-war transfer to NIH of the medical research projects sponsored by the war-time Office of Scientific Research and Development.

The creation of the other categorical Institutes then followed in rapid order. In 1948, the National Heart Institute and the National Institute of Dental Research were created by acts of Congress which, at the same time,

created NIH's present 'federal' structure and changed its collective name to the National Institutes of Health. A National Microbiological Institute was administratively established--its name subsequently changed to the National Institute of Allergy and Infectious Diseases as its scope enlarged. In 1949, the Division of Mental Hygiene became the National Institute of Mental Health. In 1950, the Congress created the National Institute of Neurological Diseases and Blindness and the National Institute of Arthritis and Metabolic Diseases.

This organizational growth was accompanied by considerable construction at the Bethesda campus, which was largely completed by 1953, and appropriate increases in the direct operating budget. There was, however, little program expansion during the period from 1950 to 1956. While this was partly due to the Korean war, it was also, necessarily, a period during which the new Institutes clarified their goals and program policies, the grant mechanisms were refined, and a stable administrative apparatus was developed.

The grant mechanisms were well established and the direct operation was well underway by fiscal year 1956. The eight appropriations for the National Institutes of Health for that year totaled $98.5 million of which $40.5 million was for research grants and contracts, $17.3 million for training grants and fellowships, $28.3 million for direct research, $10.4 for public health and disease control activities, and $2 million for administration and program management.

The next step in the evolution of the NIH programs was the passage of the Health Research Facilities Act in 1956. This legislation, which authorized matching grants for the construction of research facilities, rounded-out the scope of NIH activities so that they then had available the essential mechanisms for the development of a meaningful research program national in scope. NIH could finance the training of scientists, it could enhance the physical plant available for scientific endeavors, and it could provide for the assistance needed in the actual conduct of research.

The period of rapid program expansion was initiated after Mr. Folsom, who was then Secretary of HEW and deeply concerned about the consequences of serious illness, undertook a through inquiry into the quality of the NIH operations, the opportunities that existed for productive research, and the level of funding that would be required to take advantage of these opportunities. He came to the conclusion that the expansion of productive research should be limited only by the number of highly qualified scientists available. He proposed substantial increases in the NIH budget both for research and for training. The Congress immediately responded to his assessments of the problem. Broad program needs were immediately satisfied and from 1957 through 1963 the appropriations were increased, largely as the result of specific actions by the Congress, at a rate of approximately 30 percent per year.

For 1965--which ended a decade of very rapid, across-the-board growth--the cost of this new complex enterprise was at the level of $1.0 billion.

Program development since 1964 has not been as general or undifferentiated as it was during the period of rapid growth. Rather, this has been a period of selective development. The NIH has continued to support activities which, by scientific consensus, are considered to be important and relevant to serious disease problems. However, increasing staff time has been devoted to an analysis of the ongoing activities and deficient areas have been identified for special examination. Frequently it was found that these areas, though important, were not progressing because they did not conveniently fit into the usual discipline orientation of the academic enterprise. It became increasingly obvious that special organizational arrangements would be required if such fields were to flourish and to make their contributions to the complex of problems under study.

Some of these program-problem areas are of sufficient importance to list as examples of this programming activity

... special problems that relate to the causation of cancer and the intimate interplay of chemicals and viruses.

... problems of aging, many of which involve complex and inter-acting biological, psychological, environmental and social factors;

... research in pharmacology and toxicology which is concerned not only with drug development and the identification and prevention of adverse reactions but also with the problems created by the increasing chemical hostility of the man-made environment in which we live;

... development of the dental sciences to establish a

sounder scientific base for the advancement of modern dentistry;

... problems of child development which, as in the case of aging, involve a complex array of factors and which can result in life-long disabilities and vulnerabilities to disease;

... and, finally, greater emphasis on special problems, such as deafness, and disease that can result in blindness.

The definition of some of these broad problem areas led, very early, to the establishment of two non-categorical Institutes which became operational in 1963. The National Institute of Child Health and Human Development was established because the categorical, disease-oriented approach of the other Institutes is not well-suited to the exploration of problems that involve the complex problem of human development. A more comprehensive research approach is needed to shed light on this cycle of experience that is common to every human being. The creation of the National Institute of General Medical Sciences recognized that the categorical programs of the major Institutes depend on many common areas of science no one of which could properly be assigned to an individual Institute. The vigor of these common areas is an important determinant of progress in each and every Institute. The new Institute was assigned responsibility for the health and vigor of these common scientific undertakings.

A new dimension was added to NIH by the Heart Disease, Cancer and Stroke Amendments of 1965. The assignment to NIH of responsibility for the Regional Medical Programs constitutes a major commitment to a program that, though research-related, is primarily service-oriented.

This program grew out of the recommendations of the President's Commission on Heart Disease, Cancer and Stroke. Simply stated, its purpose is to extend the excellence of medicine in the academic setting to the primary battle ground of disease where it occurs, that is, in a community setting.

The present programs of the National Institutes of Health now comprise five major groups of activities:

(1) the conduct of research in its own laboratory and clinical facilities, and through contracts with other institutions;

(2) the support of research through grants;

(3) the support of training of highly-qualified scientists and clinicians for research careers;

(4) the construction of health research facilities through matching grants; and

(5) the support of a national program to strengthen medical service through the new Regional Medical Programs.

I should like to indicate briefly how the $1,187,250,000 requested for FY 1968 is distributed among these five major activities.

Funds for direct operations include $81.7 million for the conduct of research by the NIH scientific staff and $113 million for goal-oriented collaborative projects; $644.5 million is for the support of research through grants; $194.6 million is for the training grants and fellowship programs; $35 million is requested for the Health Research Facilities Construction program; $59.4 million is for the new Regional Medical Program. $59 million is for other direct operations, program direction, administrative services and management of the extramural program.

The bulk of the NIH budget is devoted to the support of medical research, and closely related activities, outside its own laboratories and clinics. However,funds requested for research conducted by NIH itself, are substantial. The extraordinary excellence of these scientific activities set the level of excellence of the whole enterprise. The competence and personal involvement of NIH scientists with the major research problems on the frontiers of the biosciences provides a solid and indispensable intellectual base for all other NIH activities.

The Committee will wish to consider what role the NIH programs should play in the future and to decide-- in the light of the testimony for the various Institutes--what activities are desirable and feasible. I am confident that the amounts requested in the budget estimates will be amply justified by the benefits that will accrue to the people of this country from the activities that these funds will support.

TAB 87

NATIONAL INSTITUTES OF HEALTH

Industrial Research
January 1967

by **Dr. James A. Shannon,** *director, National Institutes of Health, Dept. of Health, Education & Welfare*

PUBLIC AND PRIVATE expenditures for medical and health-related research in the United States totaled $2.1-billion in 1966. At this level, medical research activities comprise one quarter of 1% of the gross national product and nearly 5% of the $43-billion spent for all health and medical-care purposes. Obviously, the scientific attack upon health problems is a significant element in the life and affairs of the nation.

The research programs of the National Institutes of Health have grown steadily since the end of World War II, reaching a total of $800-million in the President's budget request for the current year. These programs have yielded improved methods for the prevention, diagnosis, and therapy for infectious, degenerative, neurological, and genetic diseases.

These achievements resulted from the work of a large number of academic and research institutions throughout the country as well as from the work of the laboratories of the component institutes of NIH. A variety of support mechanisms have evolved which range from collaborative programs for the conduct of specific research tasks to the general support of academic and scientific research institutions for development of excellence in their research or for construction and equipping of research facilties.

Looking to the future, the same basic pattern will be reflected in NIH research programs, but there will be special emphasis upon a series of problem areas requiring immediate attention. These will include:

■ The continued support of the broad base of independent science in the academic and research institutions of the country as deemed important and relevant to health problems. Judgments on such issues will continue to be sought from the most knowledgeable representatives of our biomedical research community.

■ Special efforts to expand and enhance the character, magnitude and emphasis of the research and training programs of academic and research institutions in the areas considered critical to the achievement of national objectives.

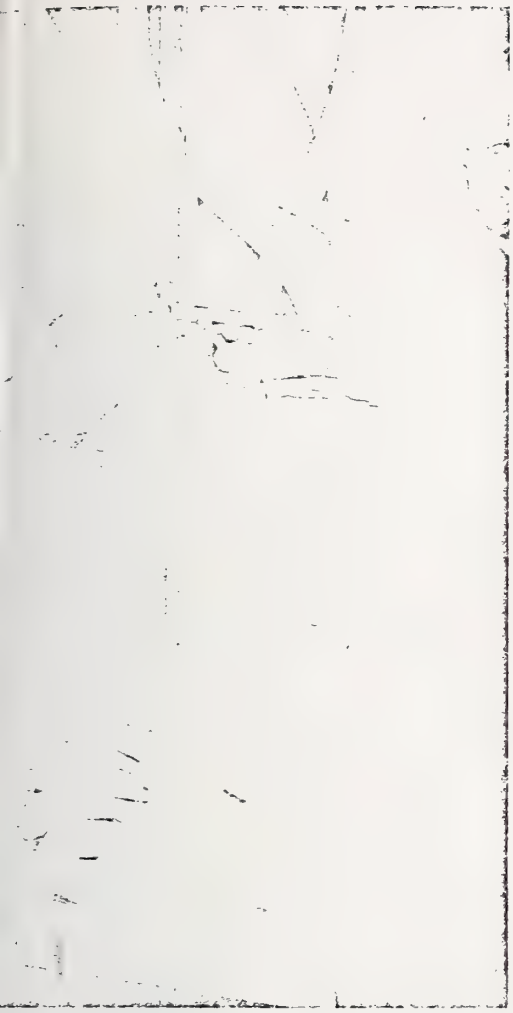
These areas include—aging, human development and mental retardation, environmental health, visual disorders, fertility control and population dynamics, pharmacology and toxicology, biomedical engineering and computer research and technology.

■ Emphasis also will be placed on the exploration of research and technological opportunities through the initiation or expansion of centrally managed research programs in such fields as the continued search for specific chemotherapeutic agents effective against leukemia, the lymphomas, breast cancer and other solid tumors; the purposeful pursuit of the viral origin of cancer, exploration of drugs for the retardation of atherosclerosis, and development of vaccines effective against a broadening array of infectious conditions.

■ Further examination into the control of kidney disease and the improvement of artificial kidneys, plus the exploration of myocardial infarction and development of heart-assist devices are areas of importance.

■ Inquiry into the cause of hypertension and its relationship to therapeutic control, and the extension of organ

SUPPORT OF MEDICAL *research places high on National Institutes of Health program. But private industry also helps with programs of its own. Here, University of Kentucky's College of Medicine doctors and IBM Corp. engineers are developing experimental nonimplantable heart ventricle and electronic control unit.*



ARTIFICIAL HEART *prototype from Goodyear Tire & Rubber Co. shown in the picture above is one of many artificial organs being developed through medical research today. Function of the middle ear is tested at Syracuse University laboratories for acoustic impedance with a Swislocki acoustic bridge shown in the photograph below.*

transplant capability and the control of the associated immunologic rejection phenomena also are scheduled for more emphatic research.

Lack theoretical base

The biomedical sciences have not achieved a general theoretical base or a logical unity which permits the merger of the various biological and medical branches. Partly owing to this shortcoming, it is impossible to accurately forecast the course of future research and the production of important research results. It is possible, however, to identify the factors which will shape the NIH research program for the next few years.

The character of future NIH efforts will be conditioned by several factors including the dynamics of academic science and graduate education, emergence of new science, new insights into disease phenomena, and opportunities for more deliberately organized research programs. Advancing the excellence of the intramural research program, further evolution of the NIH advisory structure, and improved communications to the community setting also will bear heavily on the institute's future.

The programs of NIH are constantly evolving in response to changing problems and new opportunities. It is also possible to give some examples of new areas of inquiry that should be representative of future efforts. To some degree, the new and expanded programs reveal a trend toward increasing degrees of program management. These steps toward specific objectives becomes possible when there is sufficient scientific knowledge to serve as a foundation.

The growing scientific base for such programs permits selection of a limited number of program areas important to the nation's health. The following examples are objectives selected for special attention.

■ In the Allergy & Infectious Diseases Institute, respiratory virus vaccine development; the study of body reactions blocking transplantation of organs and tissues; rubella vaccine development with a view to the general prevention of German measles; production of standard reagents for purposes of reference in research; and expanded research on tuberculosis, emphysema, and hepatitis.

■ In the Arthritis & Metabolic Diseases Institute, further development of an artificial kidney, the study of the possible role of such viable agents as the L-forms of bacteria in rheumatic diseases, and the extension of research on diabetes to the early detection of the pre-diabetic state are being closely examined.

■ In the Cancer Institute, the investigation of viral involvement in human and animal leukemia and the attempt to develop anti-leukemia vaccines are considered very important. The possible extension of the leukemia model system to solid tumors also is being studied.

■ In the Institute of Dental Research, expanded study on the prevention of tooth decay and predisposing factors to peridontal disease and oral cancer are two major areas of increased activity. Improved biomaterials is also of interest, particularly the development of an adhesive substance for dental restorations.

■ In the National Institute of Mental Health, enlarged programs in suicide prevention, alcoholism, drug abuse, child mental illness, and crime and delinquency are being enlarged.

■ In the National Heart Institute, artificial heart support devices which reflect current knowledge of circulatory physiology and design engineering, are looking toward improvement. An 8-year heart drug study is underway, and improved methods of fractionating whole blood and storing specific blood elements are currently in development.

■ In the National Institute of Neurological Diseases & Blindness, the completion of a highly programed investigation of the long-term results of incidences during pregnancy and delivery is in progress. Expanded studies are being conducted on visual disorders, epilepsy, head injury, stroke, and latent viruses in the cause of neurologic diseases.

Sophisticated disciplines

It can be seen from the foregoing examples that a number of the new programs of NIH are sharply focused on causal factors of disease which at present are subtle and elusive, ranging from slow-acting viruses to faulty immunologic mechanisms.

Similarly, the new approaches to prevention, diagnosis, and therapy will include sophisticated disciplines and agents, from molecular genetics to virus vaccines. Such operations will draw heavily upon emerging disciplines of a complex character, including biomathematics, bioengineering, computer science, physical chemistry, and molecular biology.

The preparation for such programs necessarily will include the development of resources—trained personnel, research facilities, and special resources. Thus, the new programs will make new demands upon NIH activities serving older program areas, and will draw upon the overall planning and administrative functions in challenging new ways. ■

TAB 88

FOR RELEASE AFTER 1 P.M.

MONDAY, JANUARY 16, 1967

"SCIENCE AND SERVICE"*

*As delivered by: James A. Shannon, M.D.
Director
National Institutes of Health
Bethesda, Maryland

Delivered on: January 16, 1967 (1 p.m.)

Delivered at: Conference on Regional
Medical Programs at
Washington Hilton Hotel
Washington, D.C.
January 15-17, 1967

"SCIENCE AND SERVICE"

James A. Shannon, M.D.
Director
National Institutes of Health

There was an article in a recent issue of Science on "The Art of Talking about Science." The author--a distinguished British scientist--discussed the oral transmission of scientific information in a manner that is devastating to those, like myself, who have been called on to make general presentations. His penetrating comments on the foibles of lecturers are even more apt for those who would engage in a luncheon talk such as I am about to make.

In fact, I was embarrassed to read this article at the very time that I was preparing for today.

* * *

His first rule is that a 'talk'--as distinguished from a 'paper'--should never be read. He made the point that it is simpler to read than to listen and understand--implying, though not precisely saying, that to take the time of a captive audience to read a dissertation came perilously close to insulting their intelligence even though the thoughts expressed are suitably profound. He argued that to deliver a tightly argued thesis in well-rounded phrases conceived in the leisure of one's study does not give the audience time to think and is like asking a friend to go for a walk while you drive along beside him in a car.

* * *

His second point was that only a limited number of points should be made, and few of these should be supported in great detail. He suggested that the effectiveness of the discussion could best be judged by the extent to which the selected points presented could be recalled when talking to one's wife or husband at breakfast the following day.

I suppose that the author has heard, as have you and I, many brilliant presentations of complex subjects in which a clever and intelligent speaker builds a complex structure in a manner that enables us

. . . to follow the construction point by point,

. . . to understand the transition from one level of complexity to another, and

. . . to have the feeling, at its end, that we have participated in a satisfying intellectual tour de force

only to find the next day that we really do not remember much about the presentation except the name of the lecturer, the title of his talk, and the brilliance of his performance. At best we may remember some of the major points made but not the logic of the setting in which they were contained nor the way in which the major threads of thought were woven into a significant and logical pattern. I suspect that, if we remember the pattern at all, it is because it may be inferred from the title.

* * *

The author recommends that a lecture be loosely constructed of few parts. It should

. . . start from a base of knowledge shared by the audience,

. . . build the basic structure of thought,

. . . provide for its elaboration within the time available, and

. . . most importantly, allow time to summarize the major thoughts or ideas one wishes the audience to retain.

* * *

The author made many other pertinent points but one I remember very well. He recalled the practice of the Royal Institution, from whose directorship he has just retired, of giving the speaker some 30 or 40 minutes of solitude prior to his discussion--even to the extent of placing a guard at the door to prevent any intrusion into the privacy of the speaker's thoughts as he composed himself for his presentation.

Now I want to make three points.

1. The article is commended to you for reading--it is serious but presented in a light, readable fashion;
2. the adoption of its principles would make for less slumber during presentations such as this; and, finally,
3. my inability to match what he considers the minimal excellence of performance can be rationalized, in part, by my inability to have the 30 minutes or so of solitude which he so strongly recommends.

I shall, however, in a rather halting fashion, attempt to abide by some of his imperatives. Incidentally, he was not opposed to the use of notes.

* * * * * * *

I shall start from a common base of understanding.

* * *

Mecical services at the community level have a lesser degree of perfection than would be possible if all the available information were at the disposal of the physician treating the individual patient and if the physician was supported by all the diagnostic and therapeutic resources that are needed to apply this body of old and recent information to the problems presented.

* * *

A further point of general understanding is our common appreciation of the fact that in our advanced institutions, especially in our better university hospitals, there is little useful knowledge lying undisclosed in laboratory note-books or unread in journals and books in the library. Knowledge that can help to solve a patient's problems is, indeed, utilized in the day-to-day work of university-based physicians in such a medical center.

However, a comparable situation does not exist in many communities--though I do not say all--where the physician has been out of the mainstream of learning for a considerable period of time and where the diagnostic and therapeutic resources are less than optimal.

* * *

The next relevant fact is that through legislation--and particularly through Titles 18 and 19 of the Social Security Amendments of 1965--the nation has asserted that each individual has a right to superior medical care and has begun to provide, through many Federal, State and private mechanisms, for payment systems by which this right may be secured. We are agreed, however, that such systems must not interfere with our general private base for the delivery of medical services. It is the national purpose to correct deficiencies in the delivery of medical services by using the present system as the core structure for social embellishment rather than by attempting to build a new system.

* * * * * * * * * *

Finally--and still within our base of common understanding--you are with us for a few days to examine the circumstances developing in relation to the Regional Medical Programs in order to determine how, within a broad segment of medicine, certain moves be made, in accordance with the intentions of the law, to facilitate the development of excellence in our handling of a series of so-called dread diseases--heart disease, cancer, stroke and related medical disabilities. You will be asked to comment, for the ultimate benefit of the President and the Congress, on the adequacies of the initial moves that are now being made or that are immediately in prospect. You will also be asked to anticipate some of the problems, assess the likelihood of success of current strategy, and on this basis, advise the Division of Regional Medical Programs on how they may best project their action into the immediate future.

* * *

More importantly, you will be asked to assess, on the basis of an informed professional judgment, the extent to which the Division should seek simple extension of present legislative authority or seek its modification in order to heighten the prospect of success for the program.

Now, you will not be asked at this time for specific recommendations but, in view of the complexity of the undertaking, to comment on the problems of applying the proposed strategy to your own regional situation whether this be rural or metropolitan and whether it be rich or poor in medical resources.

A sifting of your informed discussion will be a major input of information to the National Advisory Council which will advise and to the Division which must act.

* * * * * * *

You may well ask, at this point, "Of what concern is all this to the NIH?"--an organization which, in recent years, has been largely concerned with the development of new knowledge rather than the delivery of services.

* * *

One can give either of two answers to such a question--either would appear to be correct and, indeed, each is in fact partially correct.

The first answer would be that the creation of Regional Medical Programs permits a large social experiment to determine what is needed to facilitate the rapid use of available knowledge in the solution of serious disease problems in the setting in which these problems generally occur--that is, in a typical community. In this sense it is straight forward operational research.

The second answer reflects the fact that in the best of our university medical centers we have a unique mix of professional talents. This consists of scientists engaged in fundamental research, physicians eagerly attempting to apply such fundamental information to the solution of disease problems, and physicians primarily concerned with the problems of medical care and the education of young physicians. This combination of skills and interests makes possible the delivery of medical services in a professional setting that approaches the ideal. It is in such a setting that the best of medical services <u>are</u> delivered or <u>can be</u> delivered. The problem is to determine how such know-how and such excellence can be exported for use by the community at large. Or, to put it another way, how can the university-type hospital--and there are many of these that are not, in fact, part of or closely associated with a university or medical school--how can such an institution yield the isolation that protects and fosters scholarly activity and assume a larger social function without, at the same time, placing in jeopardy its present purposes.

* * *

As the one single institution most concerned with these present purposes--that is research and education--the NIH has been given the task of working with groups, such as you, in developing programs, suitable for regions of quite diverse character and medical resources, that will

. . . preserve the excellence of the present programs, and, indeed, foster and develop institutional excellence in science and education where it is now lacking,

. . . provide for the discharge of a largely new social responsibility in a manner that will strengthen, rather than weaken, the current institutional programs, and

. . . provide, under suitable auspices, for the linkage between these science based programs and the community apparatus within which medical services are delivered.

We believe that we can do the first of these three--given adequate funds. We look to you to help us do the latter, two.

Let me hasten to add that, in our view, the full elaboration of the new mechanisms we seek will not be achieved in a year or two.

* * *

We also expect that not all of your strivings will be successful. There will come a time in some--and, perhaps, many--of your programs when it will be more appropriate to take your losses and begin anew, profiting by your own experiences and those of others. If this were not the case, our problems and yours would be very simple. Unfortunately they are not.

* * *

The problem will be made both more difficult and more urgent by the rapid evolution of the medical scene. I believe that we are fast entering a period of really rapid pay-off from our large investment in the biomedical sciences. Advances have been substantial in the past two decades but they are only a harbinger of what is to come.

The biomedical science establishment, in its present magnitude and diversity, is something less than 5 years old. This is a fact that is frequently overlooked. However, scientists now capable of entering the field, at either the laboratory or clinical level, are better trained and generally more capable than was true heretofore. It is predictable that as the biomedical sciences move from the empiricism so characteristic of the past to the clarification and generalization of our understanding of biological phenomenon, their impact on the day-to-day happenings in medicine will be profound.

This transition will result in an even higher rate of professional obsolescence for practicing physicians and will require a much more purposeful system of professional renewal in the future than in the past.

* * *

And this brings me to my final point. Each regional advisory group must concern itself as much with the maintenance of the professional capabilities of local physicians in a rapidly changing and increasingly complex situation as with arrangements for improving the support for and utilization of these capabilities.

* * *

Now, following my British mentor's advice, I shall remind you of the points I would have you remember.

1. The delivery of services is less than optimal for many segments of our population.
2. The financial barriers to good services are being rapidly removed as a consequence of state and national judgments that every individual has a right to excellence in the medical care he requires.
3. In a privately-based system for the delivery of medical services, general excellence is now most frequently found in a situation where there is a mix of science, education and service.
4. Although we must contend with many diverse geographic and social circumstances, NIH, in administering the Regional Medical Programs, will strive to preserve existing centers of excellence in science, education and service while, at the same time, working with State and local forces, evolve a system that will make available to the bulk of the population medical services that are excellent in quality and adequate in quantity--at least in a major segment of the diseases that plague us all.

* * *

NIH does not have the responsibility of achieving these desirable ends alone but in conjunction with a series of other programs with similar objectives. But I believe that the Regional Medical Programs, properly developed, is the keystone of a structure which will permit the delivery of the type of medical care services we all desire.

TAB 89

Medicine Moves Toward Systems Techniques

JAMES A. SHANNON

THIS DECADE HAS SEEN the broad incorporation of much applied mathematics and physical science into the investigative fields of biology and medicine, to the striking advantage of the biological sciences. The changes have taken place within a common forum, the university, which contains the major centers of excellence of science in both general areas.

During recent years, there has been a growing appreciation that comparable rapport should take place between engineering, broadly defined, and biology, broadly defined. A rapport has indeed evolved in several important areas, but not as a general phenomenon. This, I believe, is the result of several discrete factors: the continued imprecise nature of knowledge in biology, the quite recent emergence of opportunities for goal-oriented research of interest to the engineering professions, the location of excellence in biology at the university and the location of engineering strengths in industry, and the lack of suitable coupling mechanisms that facilitate cooperation across the interface separating biological and engineering activities.

There has indeed been good and increasing cooperation between the two in several fields such as instrument development to satisfy specific needs, and the plication of computer science and technology to problems of biology and medicine.

There has not been effective cooperation in many other areas, particularly those that involve the highly sophisticated techniques dealing with planned, concurrent and sequentially operated activities directed toward definable goals. These are skills that physical scientists and engineers bring to the problems of the aeronautics and astronautics industry. Such skills can indeed find expression in many complex problems of health care, but not, I believe, in too many complex problems in biomedicine, at the present stage of our knowledge of biological systems.

For a very long time it has been clear that a systems approach is badly needed in four prosaic but socially and economically important fields: hospital management, including the gamut of functions that extend from business management to patient management; hospital systems, including the mix of general and specialized services; systems that cover the totality of community-based delivery of medical services; and, generally speaking, the useful handling of the information. I am glad to say that at least a beginning has been made in each of these areas, the state of the engineering arts being quite capable of immediate contributions.

Types of Automation

A systems approach is also needed in a number of very attractive fields of somewhat greater complexity. The first I would touch on is the *automation of complex instrumentation.* Here, I do not have in mind the next generation of automated mass spectrometers and similarly complex measuring devices for the research laboratory. These are needed, but I believe mechanisms for their development are available. Rather, I have in mind the automation of simple but multiple chemical, cellular and serological tests that provide the hard data upon which diagnosis and assessment of medical progress is based.

As these clinical laboratory procedures become more numerous and more complicated, it is becoming increasingly difficult for a hospital to maintain the professional staff with all the skills needed to acqure valid and necessary information for a number of clinical judgments. It seems likely that progress in this field will ultimately require central facilities serving a number of hospitals that, as a result of volume in the aggregate, can support proficient technologists. Such a situation will or should provide the incentive for development of broad and sophisticated automated systems.

The second field is that of *automation for diagnostic and therapeutic purposes* of the many low-frequency outputs of various clinically significant physiological systems. Here, in addition to the conventional EKG and ECG, which too many people are concerned with, I have in mind as examples the development of a capability for handling the total electrical activity of the heart by processing techniques that will convert such electrical measurements to simple statements of electrical status that can be understood by the clinician. Also, I have in mind the development of suitable transducers and data-processing devices that can continuously or repetitively portray such functions as cardiac output or blood flow in specific parts of the circulation. These are mentioned as examples of current possibilities and needs in bioengineering, but not by way of exploring fields in any depth.

So far we have dealt with relatively simple biological systems and with problems that can be rather precisely defined by the biologist to the engineer. I should like to move to somewhat more complex fields, where one begins to find limitations in this matter of communication. Here, too, we move from a largely developmental field to one of fundamental research. To work on problems of this nature, the engineer must acquire some definitive knowledge and understanding of the biological processes involved and the biologist, some insight into relevant engineering capabilities.

I am thinking here of very complex problems involving information, communication and control in biological systems. Study of these will involve the application and extension, for example, of information theory to biologi-

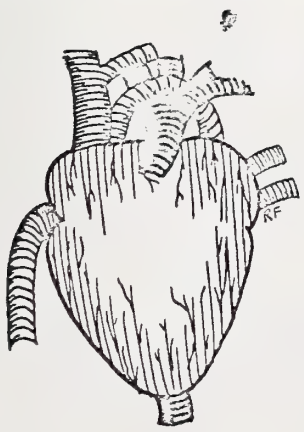
cal systems. The field includes human control mechanisms, the application of mathematical methods for the analysis of nonlinear systems, and model-building with computer simulation of biological control systems. In a very broad sense it can encompass such diverse program reas as machine linguistics on the one nand and the complex operations of the central nervous system on the other.

These are examples of quite advanced areas of scientific inquiry requiring the most sophisticated and, indeed, most challenging efforts of the engineer in biology.

We see, then, that the interface across which the engineer-scientist and the biologist can interact is a broad one encompassing some objectives that are quite feasible today and of great fundamental importance, and others of such complexity that they will not yield to any but the most brilliant minds available. But all this is fairly abstract—that is to say, lacking in the sense of the practical, which I am told is one of the engineer's identifying characteristics. With this in mind, let me touch on two areas that highlight the biological complexity common to many problems of interest to the engineer and the biologist.

The two fields I refer to are attractive not only to the biologist and the engineer, but also to the lay science writer. They concern the general problem of rgan replacement, specifically the heart .nd the kidney. The newspapers as well as the scientific journals have kept pace with our joint feeble efforts to replace the heart with a mechanical device or to supplement the kidney, the lung or the liver with functions provided by physical machines.

Perhaps the most striking of these potential *"tours de force"* is the development of an artificial heart. This is an interesting subject for initial consideration. During the 1940's and '50's pump aerator devices were developed by engineers, physiologists and surgeons



that can take over the combined functions of the heart and lungs for limited periods of time. This advance, together with better diagnosis and support procedures and more advanced surgical skills, is the basis for open-heart surgery as we know it today. Essential to the development were a variety of engineering devices that are required to monitor the physiological status of the patient during and after the surgical procedures and others that can produce relative quiescence of the heart during manipulation, can interrupt arrhythmias, and can reinstitute a regular rhythm at the option of the surgeon.

'Go-for-broke' Approach

Thus it was clearly apparent to many physicians and surgeons by the late 1950's that it should be possible to provide temporary or permanent assist or replacement devices to supplement or replace an inadequate myocardium (heart) if this was judged desirable. Viewed from the standpoint of the engineer, this was considered a complex but not an impossible undertaking for four reasons:

—The pressure and flow relationship for both the pulmonary and systemic circulations could be defined with fair precision.

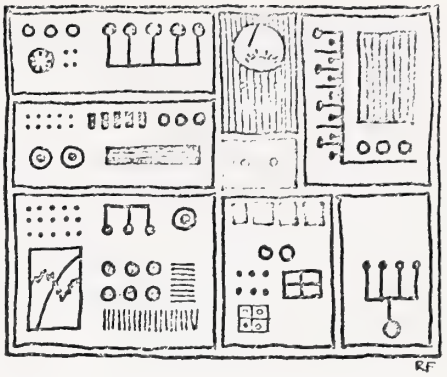
—The variations of these in response to stress could be precisely established.

—The energy requirements were definable.

—There was some information on which to base a further study of materials suitable for implantation.

With such a background, and with a sense of urgency because of the breadth of the medical problem, the National Advisory Heart Council requested the National Heart Institute in 1964 to explore the feasibility of establishing a broad program, developmental in nature, aimed at acquiring a mechanical device that might serve as a replacement for an inadequate heart. The Council, appreciating the complexity of the problem, recommended applying the techniques of the systems approach to the development of components as well as their incorporation into a system.

Appropriate individuals were brought together to consider the problem. They advised a "go-for-broke" approach aimed at developing a mechanical replacement of the heart, with suitable servo control mechanisms to provide adequately for various levels of physiological effort. The device was to be implanted, together with its energy supply and control mechanisms. And there were to be spin-offs in the form of a series of assist devices for emergency, temporary or permanent assistance to cardiac function.



Medically, it was considered that if such devices were available they might be utilized in a variety of clinical conditions, but would be most useful in the case of myocardial infarction and the resulting myocardical insufficiency. However, it was also clearly apparent on dispassionate examination that even if such devices were available, the clinician does not have the information upon which to base judgments on when one of the devices is needed or which of them could best satisfy the situation.

In other words, our understanding of the consequences of myocardial infarction is inadequate to permit a decision on the likely further course of an individual case at the point in time when a definitive decision to use or not to use such a device must be made. It is clearly apparent that the majority of patients with infarction who die do so in the first 48 hours. But this is usually because of cardiac arrest or arrhythmias rather than because of myocardial insufficiency. It is also clear that even in the early critical period of the first seven to ten days, one cannot predict what the eventual cardiac disability will be—whether maximal benefit would be obtained from a left ventricular assist device or whether total replacement would be required to provide for adequate cardiac function, or whether neither would be advantageous.

With such problems in mind, however, the simple fact of the matter is this: Even if devices were available that had solved all the problems of the living-nonliving interface—looking inward to the blood and outward to the containing tissue, and the problems of pumps, their energizers and their physiological control—a critical determination to use a specific device could still not be made with confidence. But with the problems more precisely defined in a limited series of patients fitted with left ventricular assist devices because of manifest inadequacy of the myocardium, a systematic program can now be developed.

High Costs of Treatment

The first stage of the program will relate to a purposeful clarification of

the course of myocardial insufficiency, particularly that resulting from infarction and the utility of support devices of a relatively simple nature. These studies will define in broad outline the course of the myocardial infarction in a statistically satisfactory manner, with measurements that will hopefully provide for more adequate prediction of the likelihood of myocardial insufficiency and its extent.

The studies should also indicate the use and characteristics of effective left ventricular assist devices in a limited number of highly sophisticated settings to define their capability, first when used in a temporary fashion and later in a permanent setting if necessary.

Such studies should define not only the desirable characteristics of assist devices and the patients who will benefit by their placement, but also the residue of cardiac damage that may require more extensive support. Concurrent studies on materiel, power sources, pumps and control mechanisms to provide mobility can also be planned. In relation to time, I should guess that this phase of the study most certainly will require three to five years of systematic work before further judgments of program development can be considered.

In another type of organ replacement, one finds somewhat similar problems. I have in mind the development of simplified forms of hemodialysis, or blood separation, in kidney treatment. Here again, the biological problems are far more complex than those that relate to engineering development.

At present there is no doubt that hemodialysis is a lifesaving measure which can sustain patients with complete chronic kidney failure in almost normal activity for very long periods of time. The equipment now in use is not perfect, and there are many other major problems—for example, problems of infection at the transcutaneous point of entrance into the vascular system and problems of vessel thrombosis.

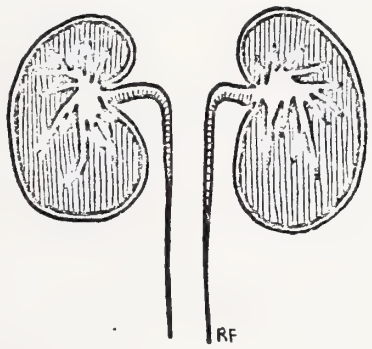
Actually, if all the medical problems were solved, we still would not have a technique that is economically feasible for use in all cases of kidney insufficiency. The current cost per patient per year is about $10,000, and the complex nature of the setting in which the procedure can be employed safely precludes—at least to my mind—its widespread use in the present state.

It is because of these facts that we are undertaking a developmental program to simplify both the device and the circumstances of its use. The extent to which this can be accomplished is not yet clear, except that substantial simplification is feasible.

Meanwhile, dialysis is an essential research tool that can lead to a definition of the specific biological factors that precipitate disability and death in chronic kidney insufficiency. Clarification of these factors will greatly modify the next generation of devices. This objective has been brought closer by the recent demonstration of the effectiveness of diets limited to the minimal amounts of essential amino acids as the total source of nitrogen. Hopefully, this will permit definitive study of the origin and nature of the toxic substances that result in uremia and the consequent development of more specific methods than general dialysis as a means for their removal.

Government Role Vital

Work in other fields relating to a clarification of the role of tissue antigens in the immunological rejection of organ transplants will lead to more general use of kidney transplants, thus lessening the need to resort to hemodialysis. This, though, will not be the final answer to the problem of chronic kidney insufficiency, since the condition largely results from two major diseases, pyelonephritis or glomerulonephritis.



The latter disease tends to attack the fresh kidney transplant, destroying the new organ even as it had destroyed the original organ.

So one is driven back to the need to define the cause of glomerulonephritis, determine the factors concerned with its progression and, hopefully, with such information, prevent or modify the course of the disease. Here, too, there is progress to report in the recent development of an animal model system, occurring in a strain of hybrid mice, which for the first time offers substantial opportunities for systematic study of the disease in the laboratory.

These two developments—the artificial heart and the artificial kidney—illustrate the complex biological setting in which many practical considerations of an engineering nature are found. They were cited to illuminate the problem, not to discourage the engineer.

I am convinced that biology and engineering are now developing—indeed have developed—a very broad interface. This situation, in turn, is producing close and productive contact between diverse biologists and engineers. Proper exploitation of this relationship through suitable coupling mechanisms will have profound effects on our understanding of disease, our ability to modify the consequences of disease, our understanding of many life processes and their control, our capacity for more precise diagnosis and treatment, our ability to manage our hospitals and, finally, our ability to develop more rational systems of patient care. Yet, the rate at which these objectives can be achieved depends in large measure on the ability of biology to acquire the extraordinary competencies contained in the aerospace industry.

Domestic problems of a social nature and foreign problems of a political nature are requiring such large expenditures by the Federal Government that any immediate increase in funding to support broad-scale development of biomedical engineering techniques is not likely to occur. The lack of such financial support is the major obstacle to tapping the substantial resources of the aerospace industry, which has already indicated an eagerness and willingness to attack a portion of medical problems. The activities I have discussed here are expensive; except for problems of automation, there is little likelihood of developing substantial capital investment by industry.

Consequently, the extent of this industry's engagement in problems of medicine will likely be dominated by the Federal Government's ability to provide more or less complete support for the activities undertaken. It is my strong hope that the restraints on Federal budgeting for this purpose are only temporary, since both from the standpoints of a very vigorous industry and our national needs it is most important to broaden the base of the aerospace and related industries to encompass a number of fields of vast social importance.

In the meantime, at the National Institutes of Health, we intend, within such capabilities as we now possess, to provide coupling devices for those of industry who wish to join us in common effort. With such a partnership established between science and technology, we cannot fail to achieve great success in the years to come.

James A. Shannon, M.D., director of National Institutes of Health, and member of the Federal Council of Science and Technology, is internationally known for his work in tropical diseases.

TAB 90

#467

ROLE OF FUNDAMENTAL RESEARCH IN A MISSION-ORIENTED AGENCY*

James A. Shannon, M. D.**

The subject of this conference has been discussed many times, by many people, and in many forums. There is general agreement that the performance of fundamental research should be included within the functions of every agency with a technical mission. The problem is one of determining the mix of fundamental, applied and developmental research that is desirable, or of rationalizing the purpose and character of the work to be supported in the fundamental category. Having determined that this general type of activity is essential to the health and vigor of an agency program, then the question becomes one of how best to distribute the effort.

The figure (page 2) represents a bureaucratic organization you will all recognize as both hierarchical in nature and quite common in its replication in government. Actually, it is the general organization of the Public Health Service subsequent to its reorganization in 1944.

--The Agency is the Public Health Service.

--The Bureaus are the National Institutes of Health, whose primary purpose was to obtain new knowledge; the Bureau of State Services, whose purpose was to develop ways to apply both new and

---

*Presented at the Annual Meeting of the Biophysical Society, Houston, Texas, February 22-24, 1967.

**Director, National Institutes of Health, Public Health Service, U. S. Department of Health, Education, and Welfare.

existing knowledge; and the Bureau of Medical Services, whose purpose was to deliver services in several closed systems--PHS hospitals, the Indian Service, etc.

--The Divisions are the various Institutes of NIH, generally categorical in nature, as they evolved during the late 1940's and the 1950's.

--The Laboratories are those that composed the direct operations of an Institute at Bethesda about 1946-56, each reflecting activity in a general scientific discipline or in several related disciplines.

The Sections were narrowly limited and, at NIH during that period, were usually composed of the appropriate professional and technical support required for the productive activity of a single senior scientist.

Such an agency arrangement has the apparent advantage of simplicity in formulation of functions. Research was limited largely to a single agency, NIH. Substantive areas of science in terms of categorical objectives were defined by Division, specific areas of science by Laboratory, and within this by more limited individual areas of scientific activity, by Section.

Theoretically, the laboratory and sectional organization permitted a distribution of effort within a program division which had an overall objective. For example, the solution of problems of heart disease and their management would be satisfied by the development of

a mix of fundamental and applied research within the existing laboratory and sectional apparatus. And those activities could be expected to yield substantial information on the more important problems in the general area of responsibility of the Heart Institute.

Program development in such a setting--i.e., the NIH direct operation--depends largely on the selection of fields to be explored and the acquisition of suitable personnel rather than technical direction in the ordinary sense of the term.

The origins of heart diseases were few: congenital defects, rheumatic fever, arteriosclerosis and hypertension. The stake was, and is, high: in the aggregate these diseases are responsible for 50 percent of all deaths at all ages. The mission remains simple: to develop new information that could lead to the prevention, modification or cure of this series of diseases. The research problems were initially the concern of the intramural program at Bethesda; later these problems received broad attention through university research under grants-in-aid.

Research, however, was generally limited to the conventional biomedical sciences, with other elements of the National Heart Institute and of the Public Health Service left to apply the results of this new knowledge or to provide help in permitting its broad application in the practive of medicine.

Very simple, very clean-cut, no problems.

However, nothing is so simple, no organization so clean-cut, and no complex problem separable in the actual approach into such neat packages.

This is more obvious if we look--not at the organization of the Public Health Service about 1948, when the Heart Institute was established--but rather at the discernible general functions of the Service at that time. Program leadership and general services were provided from the Office of the Surgeon General, supported by a bureau structure at that level.

Such neat packaging, with an array of Institutes aligned against the major diseases by 1950, must really have warmed the hearts of the bureaucrats, and indeed the professionals. It seemed to satisfy the needs of the time--as these are too generally viewed--in terms of past needs and past deficiencies. The inadequacies, however, became apparent in the mid-fifties, leading to minor modification of organization in the late fifties and a general reorganization in 1960. The organization was progressively modified during the early sixties, leading to a second reorganization in 1966. Currently, another reorganization involving the whole health function of the Department of Health, Education, and Welfare is in prospect.

Such a serial shifting of function reflects in part the growth of the Public Health Service, but in no small measure reflects also in the 1944 organization pattern, an inadequate recognition of the essential role of research in the individual functions of the Service's mission. Moreover, it reveals that at each level and in each component, there is need for a suitable mix of both fundamental and applied research, but with the type appropriate to one bureau being substantially different from that of another.

For example:

1. There was, and always will be, a need for operational research at the agency level--that is, the Public Health Service. Some of this must be quite abstract in nature, applicable to a broad range of problems, and containing heavy elements of the social sciences. Such studies, in retrospect and in prospect, must be the basis of policy formulation in a health agency, but cannot be restricted to health functions as viewed conventionally.

Consideration of the health problem should include collateral areas of science. Models for planning should contain social, economic, and general demographic considerations, in association with purely medical ones. Only from such considerations can a society develop health goals that are reasonable, provide the mechanisms for their attainment, and sort out the priorities that must inevitably be assigned when it is obvious that all matters cannot go forward at the rate that each professional may deem desirable. (This is impossible even in the programs of the Defense Department or Space Agency.)

It is important for today's purpose to note that at this level of activity the professionals who undertake the studies must be given many degrees of freedom in what they do, and part of what is done in these areas by the social scientist will be quite fundamental in nature, since there are no simple model systems available.

2. At the bureau level, as regards the delivery of services, it would be helpful if the Public Health Service could deal more broadly

with the sociology of providing medical services. This activity in our country is executed through a private system functioning in a community setting. Until recently the activity was not seriously complicated by local, State or Federal intervention. But even in the late 1940's, pre-payment plans were beginning to influence the activities of the practitioners, the resources available to them, their patients, and the community.

Quite apart from the economics of the situation, which demanded studies of the economic base for the services to be rendered and the alternative mechanisms for support of the professional activities, certain changes in attitude were taking place--changes that were to determine how such services would be sought and accepted by the public.

It is relatively easy to set national health goals based on reasonable national statistics concerning disease and disability. It is less easy to determine--or if need be, to modify--the health goals of the individual and his family unit; to determine whether he understands the problems of disease and disability, what he is willing to pay to avoid their consequences, and what the individual may decide are essential characteristics of the service system if he is to obtain from it the maximum benefit he can afford.

It is clear as a general premise that the health goals of the population at large were high. But realistically, there is no such thing as a "population at large" in the operational sense. The

failure of some groups to use polio vaccine in the late 1950's, though it was generally available at little or no cost, illustrates the rejection of an important health aid by a substantial number of people. Similarly, fluoridation of water, for purposes of limiting dental caries, was rejected by whole communities. These events of the 1950's indicated a need for studies in economics, education broadly defined, sociology, and social anthropology. The studies should have been directed at determining why substantial population groups rejected what the medical community was convinced had merit, and how basic behavioral patterns might be altered.

Effective studies in those areas should have been free-ranging, conducted in parallel with studies of resource development and deployment in the more technical areas usually associated with medicine. I might add that such studies are only now being formulated, though it is encouraging that the need for this type of information is recognized.

While many of these problems await solution, others are rapidly reaching a critical level. It has long been urgent, for example, that we examine our social system, from both an economic and behavioral base, in relation to an increasingly complex society which, as it adjusts itself to the urgent problems of the day, finds little use for its elder citizens. Certainly there is need for comparative studies of different cultures and subcultures, utilizing techniques available to the social anthropologist, which would provide helpful information to our nation and others facing this general problem. The problem results in part from a broad socio-economic advance and in part from advances in

medical knowledge. It is a further reflection of general advance in the technical sophistication of our society.

Problems of these general types should be encompassed by the second level of governmental organization--i.e., the bureau level. Generally speaking, they can be investigated or supported by bureaus whose overt goals are highly practical and quite definable in terms of delivery of services. I would emphasize, however, that the required data will not be forthcoming from studies that are narrow and precisely goal-oriented. The knowledge base for such studies is presently inadequate, and the base itself must be purposefully expanded by the stimulation and support of quite general studies. This can be done even as available knowledge is used in the study of immediate problem areas.

I should like to turn now to some problems and attitudes that directly affect the operations of the National Institutes of Health and the programs it deems suitable for support. Few NIH professionals, and few of their counterparts in the university, would contend that the biomedical sciences possess the store of fundamental knowledge that would permit the luxury of limiting their activities to a direct onslaught on disease problems through highly applied research. I have in mind, of course, the broad disease problems that are reflected in the titles of our categorical institutes.

I must assume that some of you here reflect the attitudes considered prevalent in the physical sciences: that all biomedical science activities

are practical, applied, and/or developmental. These are not my views. I consider the resources of NIH to be effectively deployed only if they contain a mix of fundamental, applied, and developmental research. In the absence of a broad fundamental activity--some might call this basic research--the ultimate solution of our problems will not be possible.

The first general area of research I will examine is one that most investigators, even the physical scientists, will accept as fundamental. This is the area of molecular biology. A portion of this field is concerned with an understanding of the biological mechanisms that underlie the storage and retrieval of information at a cellular level, and to the systems responsible for the performance of these operations. You will all recognize at least four components of these systems: DNA, RNA, cell organelles and soluble constituents of cell protoplasm.

The solution of problems relating to the operations of these complex units in a total system is necessary to an understanding of many phenomena that are central to the problems of medicine. There is no doubt that in addition to determining clearly discernible physical characteristics in the individual, our genetic inheritance also determines in part our intellectual development and contributes to our individual susceptibility to disease. It is difficult to believe that definitive data on such diverse problems as mental retardation or rheumatoid arthritis will lead to prevention or cure of these conditions in the absence of genetic information that permits them to emerge as clinical phenomena.

At a higher level of complexity--for example, viruses and their interaction with more organized biological systems, including man--we find a comparable need for quite fundamental information. We need a better understanding of the nature of viruses themselves, their origins, their capability of biological change, and their vulnerabilities. We must have information on the intracellular growth requirements of viruses and on their intracellular reactions, which in some cases may lead to quiescent residence in a cell and in others to replication with consequent cell death. The latter in turn may result in slow, chronically progressive disease or in an acute, fulminating process and early disability or death. Viruses may also have a capability for modifying the response of groups of cells to the broad biological controls exerted by adjacent cells, by an organ or organ system, or by the body as a whole, which may limit viral multiplication, with the consequent development of a number of diseases grouped under the general designation of cancer.

Consequently, elucidation of these phenomena is essential to our understanding of the genesis of many viral diseases, perhaps including a number of forms of cancer, as just mentioned, as well as the devastating progressive diseases of the central nervous system. Such an understanding is also necessary if we are to utilize chemical or immunological methods to prevent, modify or cure these major disorders.

Again, and still within the general frame of reference of molecular biology, it is interesting to note that certain molecular defects in our genetic makeup can lead to a number of biochemical abnormalities, which in turn manifest themselves in overt disease. Common among the so-called molecular disease are sickle-cell anemia, galactosemia, phenylketonuria, and alkaptonuria.

There are other such conditions, but for our consideration here it will suffice to note that quite fundamental studies of intermediary metabolism have permitted a definition of the biochemical defect basic to some of these diseases. (It is worth noting that the studies which provided this information have been in the human subject.) The consequent metabolic disturbance and the physical and mental impairments stemming from it have also been defined. These studies have provided the basic knowledge that now permits, through dietary means, a bypassing of the metabolic defect, and thus reasonably normal human development.

And so one will find, in any area of medicine, problems that will yield only to free-ranging research of a quite fundamental nature, the performance of which may involve isolable physical or chemical systems, animal models, or the human.

I would close by saying that any complex mission-oriented agency cannot, in general, achieve its objectives in the absence of appropriate levels of support for both fundamental research and studies directly limited to the specific missions. And I would emphasize further that

such support in suitable fields will usually be required at every level of organization.

In this view, the problem of the administrator can be simply stated. He must possess the wisdom to determine, before the fact, the quantitative relationship between fundamental, applied and developmental research, as well as the qualitative characteristics of such research which will permit his agency to accomplish its mission. He must, within available resources, decide what portion must go to the development of new knowledge and what must go to services; what must go to action programs and what must be applied to resource programs looking to the future. Few people have such wisdom, and consequently it is easier to analyze the successes and failures of past performances than to predict the future with any degree of certainty, or to say what the current deployment of resources may be expected to yield in terms of the ultimate goals.

TAB 91

Statement by
James A. Shannon, M.D.
Director, NIH

Before the
Subcommittee on Government Research
Senate Government Operations Committee
March 1, 1967

Mr. Chairman and members of the Committee:

The topic the Committee has selected for these hearings is one of keen interest to all members of our national biomedical community. I am glad, therefore, to have the opportunity to participate. The "role of the Federal Government in biomedical development and application" has become, in the past year, one of the most debated and discussed aspects of science policy. My statement is longer than I would have it, yet the importance of the subject to our program argues against substantial curtailment.

The subject is a major concern for the Department of Health, Education, and Welfare, which I represent here today, and particularly for the National Institutes of Health, whose research programs are my responsibility.

I am concerned that, despite the quality and extent of discussions to date, so many misconceptions remain. This is true with respect to the nature of the issues and at a very practical level to the options that are in fact open to us. I would like the impact of discussions here to be clarifying and constructive, and the record of this Committee to date gives me confidence that they will be just that.

To meet the issues before the Committee as directly as possible, I propose to answer each of the six questions you have posed to participants;

then add a thought or two on such general issues as still seem to warrant further comment.

Your first question was whether there is a need for additional attention by Federal agencies in the field of biomedical development and applications.

My short answer here is "yes." But this, by itself, would be misleading. However we may care to label them, there are at least three distinctively different aspects of "biomedical development and application." The problems for Federal involvement differ in each; the nature of Federal programs seeking solutions also differs in each.

First is the translation of a particular research finding into improved techniques directly applicable to problems of clinical medicine or health care. Characteristically, this translation takes place in major university medical centers, certain independent hospitals, or research foundations. The program stimulus for this--to the extent it is Federal--is most likely to be one of the research or research training programs of the NIH. In the special settings required for this translation, laboratory and clinical research, clinical training, and patient care are in continuing interaction. The most important contribution to such interaction was the establishment of clinical research centers beginning about 1960. It is the effectiveness of the interchange in such settings and in the general environment of the university center that warrants the assertion that there is no "significant body of fundamental information (in the biosciences) which is stagnantly awaiting clinical

application by competent practitioners." There are few secrets in the laboratory books of our scientists or information locked up unused in the books on the shelves of our libraries.

A second element to consider is the extension of new clinical diagnostic and therapeutic techniques from the settings in which they were first devised, to their general application in health practice in local communities. Such extension calls for a number of distinctive program approaches. The problems to be met may require:

(1) provision of a system of locally based continuing education for health practitioners,

(2) provision of special resources needed at the community level for improved clinical techniques and health care,

(3) a general strengthening of the framework for delivery of health services, to offset handicapping socio-economic factors and provide for special problems of special social and ethnic groups.

Many Departmental programs and indeed some programs of other Departments--most of them new or with innovative elements built in--are addressed to meeting these needs. These include the Regional Medical Programs located in NIH; comprehensive health planning and action programs of the Public Health Service; and other community, communications, demonstration and health service programs of the Public Health Service and other Departmental components such as the Children's Bureau; and such Departmental programs concerned with financing of health care as Medicare (Title XVIII) and Medicaid (Title XIX). There are a number of others.

A third element is "biomedical development." Using the National Science Foundation definition, "development" is the systematic use of the knowledge and understanding, gained from research, directed toward the production of useful materials, devices, systems, or methods, including design and development of prototypes and processes. It is thus characterized by use of industrial technologies and may involve a systems approach. In the biosciences, this type of industrial development has made a substantial impact on drug development through the pharmaceutical industry. This industry, incidentally, is responsible for the support through private funds of about 25 percent of all national expenditures for medical research. Only in a limited number of cases has the Federal Government had programs of this kind; and a broad and comprehensive engagement by the Federal programs would depart sharply from past practices. This is true, though the starting base for a number of such programs--each of which has been carefully selected--may be identified at the NIH. These include the artificial heart and artificial kidney programs, instrument development, clinical automation, etc. The inhibiting factors in this program area are quite real, the key ones being: the acute shortage of first-rate engineering talent, either engaged in or available to the biosciences; also, and related to this, and I believe determining this, is the need for program funding at levels unprecedented in more conventional biomedical research and the certain indication of a broad growth potential for each activity.

Before a major "biomedical development" program is mounted by the Federal Government, specific reassurances are needed on these points:

(1) that the science base for the program is adequate,

(2) that necessary management skills are on hand, and organizational arrangements permit effective deployment of these skills,

(3) that the resources to carry forward the program will not be made available in adequate amount unless the Federal Government provides them.

I would also say that:

(4) each such program should be funded separately, on its own merits, and

(5) each effort should be scaled realistically in terms of the unusually high funding levels that will be required to draw in the necessary quality of contractor competence, both in engineering and management.

In summary, there is indeed need for additional Federal attention in the field of biomedical development and application. But this attention must take into account the quite different challenge and needs in the several applications areas just described. I would add that current concepts of acceptable budget levels permit little of such proposed activities. I see little to be gained and tragic loss were we to disassemble ongoing research of a fundamental and applied nature in order to take advantage of the developmental possibilities we perceive.

Your second question was: "What means are employed by your Department to establish research priorities and long-range research plans?"

If I limit the scope of my answer to the NIH--which seems appropriate in this instance--I would say that these are established by Institute Directors for their respective program areas, drawing principally on advice from the planning activities of their staffs and the counterpart planning of their National Advisory Council. I cannot stress too strongly that a prime essential of the planning process must be centered in the fulltime activity of the staff of an Institute. Given this, there then emerges a special role for the Council and for special disease- and problem-oriented committees. The operations of a National Advisory Council afford a mechanism for examining opportunities and the direction of program effort by a group of professionals not involved in the execution of the effort. National Advisory Councils typically include a number of outstanding nongovernmental scientists who have won distinction in relevant research areas, also lay members distinguished in some area of public affairs, and, finally, liaison members from other Federal agencies with major health activities. In earlier years, much Council effort was devoted to the detailed review of individual grant applications for program relevance. As the increasing volume of grant applications has made grant-by-grant review less feasible and less meaningful, it has been possible to shift the emphasis of Council activity to the broad considerations of program planning and evaluation. These changes have also been reflected in the character of Institute staffs and organization, that is, in the development of staff capability for program planning and analysis.

In addition, several new advisory mechanisms have been established--or are in the process of being established--at the NIH which contribute to the overall process of longer range planning. These include:

(1) The Advisory Committee to the Director, NIH, which performs a somewhat analagous function for total NIH program as individual National Advisory Councils perform for their respective Institutes. As the apparatus of this committee develops, it will increasingly be concerned with a consideration of long-range program direction and emphasis and consequently in the deployment of resources. The membership of this committee encompasses outstanding individuals in the fields of medicine, the physical and social sciences, and public affairs.

(2) A structure of disease or specific problem-oriented committees is being set up throughout NIH programs, with membership drawn from outstanding experts in relevant areas. A number of these have been in effective operation for some time so that we are aware of both the potential and limitations of such groups. Each Institute may have as many of these committees as are needed to cover its major disease or disciplinary concerns. Committee responsibility for its assigned area includes examining and reporting on "the state of the art," and identifying gaps in present support as well as areas warranting increased program attention.

Backing up both the Council and committee deliberations, and the decision-making apparatus in each Institute, is a framework of planning officials and supporting staff, aided by computer resources for program

analysis, which performs advisory and evaluation functions across all program areas.

A new mechanism worth noting is the program-planning-budgeting system established at the direction of the Bureau of the Budget to cover HEW and other major agency programs. The intent here is to establish a system to provide for more informed program decision making and to assess the course and effectiveness of alternative program approaches. This would be useful indeed! But much of our activity is not amenable to such an analytic approach--at least as presently worked out--and the conventional budget approach seems more meaningful for the time being. Be that as it may, we are committed to the effort, and the mechanism does have a longer range promise if the techniques can be so modified as to align them with problems of a real world.

Your third question was: "How do you evaluate and maintain a continuing examination of on-going projects and programs?"

I have already cited the role here of National Advisory Councils, of special advisory committees that relate to definable portions of Institute programs, the activities of Institute and Division planning and program staff, and the still embryonic planning-programming-budgeting system. I would add these points:

(1) All NIH research and training projects that are planned to continue over a period of years become subject to a built-in review process which periodically assesses the quality, direction, and productivity of effort as a prerequisite to extension of funding.

Such continuing assessments involve the same type of peer group evaluations as applied to initial approval of the grant application.

(2) Expert reviews by ad hoc advisory groups are periodically established to assess progress in particular program areas warranting attention for one reason or another. An example of this would be the Richardson Committee review of the cancer chemotherapy program.

(3) NIH programs as a whole have also received a series of large-scale reviews over time, beginning with the Long Committee in 1956, and extending through the Bayne-Jones, the Jones, the Wooldridge, the Kistiakowsky, and the Ruina studies. Few Federal programs have been as extensively reviewed and evaluated as the biomedical research and training programs of the National Institutes of Health. /With your permission, I will insert for the record a listing of these studies. I think you will find them interesting./

Your fourth question was: "What means does your Department use to maintain adequate communication with scientists and engineers in the field of biomedical development and applications, as well as with medical practitioners and hospitals? Are they adequate?"

I will break my discussion of communications problems into several parts:

(1) Taking the least troublesome aspect first--scientist-to-scientist communications--I would say that we do rather well here. You must appreciate that in biomedicine (unlike the defense area) there is a completely open scientific literature; also a well-developed structure

of scientist-to-scientist communication through professional scientific journals of all kinds, and through seminars, conferences, and symposia. Add to this the effective interaction provided for laboratory and clinical research, clinical training, and health care by major university medical centers, and you will appreciate why I am reasonably well satisfied--though not complacent--about this area of communications.

(2) Communication between scientists, the health practitioners, and their hospitals is much less adequate than between scientists and scientists. While the scientific literature is open in theory to health practitioners, its value for communication in this area is limited. Few practitioners can spare the time from their patients to read and absorb the quantity of literature required. Furthermore, the rapid advance of science limits the duration of knowledge acquired in the formal training process. Therefore, special devices are required to meet this problem. As I mentioned before, many Departmental programs aim at closing this science-practice communications gap. The Regional Medical Program--with its emphasis on continuing education for physicians and the establishment of operating links between university medical centers and the community health scene--is one of the more promising of these. A similar function is served by the Community Mental Health Centers program. Many other community-oriented, disease control, demonstration, and health services programs of the Public Health Service are having an impact in this problem area.

In the final analysis, there is needed a formal system of continuing education that makes information available to the practitioner in a systematic and convenient fashion. I am personally convinced that such a system, even if established, will not be generally used unless professional involvement such as periodic measurement of professional capability is built into the patient-care system.

(3) Communications between scientists and engineers represent a second area of inadequacy. Relatively few on-going programs are directed to closing this communications gap--for the practical but disturbing reason that there are all too few engineers working in the biosciences or contiguous areas. However, the bio-engineering centers program in the Division of Research Facilities and Resources and support of research and training in bio-engineering by the National Institute of General Medical Sciences will contribute to progress in this area. At this point in time, the progress will be dollar limited and consequently slow.

Your fifth question was: "What procedures do you employ to translate results of biomedical research into actual treatment and care of patients?"

The organization of American medicine is such that improvement in the delivery of health services cannot be directly brought about by the NIH or any other Federal agency. The treatment and care of patients is, in the United States, a private function with, of course, the exception of care for Federal beneficiaries such as veterans. Consequently, Federal programs

work through public or private agencies that relate to the medical care scene. In this system, the university medical center provides the setting for the transfer of research findings in biomedicine, by means of the ready access to new knowledge, and the skills, techniques, and other resources needed to apply them. To the extent that the NIH has had a major impact on the university medical center, likewise we have had an impact on this transferral process. No other area of science has a natural theater, as it were, for the transmission of the fruits of research comparable to the university center in biomedicine.

The Regional Medical Programs is the focus for NIH interest and concern in improving the delivery of health services. The British medical journal, Lancet, aptly captured the significance of the Regional Medical Programs in calling them "An American Catalyst." In this catalytic role, this regional approach to health relationships must cause such diverse elements as medical schools, health professional organizations, individual physicians, hospitals, voluntary organizations, and business and community planners to react together in a way which will promote, at the community level, the same high quality of medical care now available at our university centers of medical excellence.

One key element in the effort to translate research findings into improved patient care has emerged from the evolving Regional Medical Programs. The practicing physician is the core of our system of health care, and his continuing education and retraining in modern medical precepts is crucial to the higher quality patient care which the Regional

Medical Programs has as a goal for the Nation. The Regional Medical Programs can also provide some of the special diagnostic and therapeutic resources needed at the community level as more physicians require these complex new procedures in day-to-day practice. A general increase in professional sophistication can reduce significantly the toll of death and disability from disease.

Your final question was: "What further steps might be taken by Government agencies to bring about more biomedical development without diminishing basic research and without disorienting institutions involved in basic research?"

I believe that we in the Federal Government need only adhere to certain common-sense precepts:

(1) The first imperative is to be certain that the science base for a proposed development effort has been carefully assessed by those most competent to judge these matters, and has in fact been found adequate. Another prerequisite is to be certain that the problem to be attacked is a major one; for a lesser problem would not warrant the heavy allocation of resources that may be required. Some very promising areas are research on containment, on the automation of clinical laboratory procedures, on centrifugation techniques, on models of complex biological system, and on the computerization of hospital systems of drug dispensing, patient monitoring, and record keeping.

(2) A second set of requirements has to do with funding for developmental programs. If major new programs of this type are to be mounted "without diminishing basic research and without disorienting institutions involved in basic research," the new program must be funded on the basis of its own merits, and not as a substitute for the essential base of continuing scientific activity out of which emerge the developmental opportunities. Furthermore, the costs involved in the successful mounting and conduct of development programs must be recognized as high. Costs must, therefore, be projected realistically at levels that assure the entrance of very competent professionals into the area of activity if the objectives are to be attained and these in a reasonable time.

That concludes my direct responses to Committee questions.

I would like to add an observation or two--and perhaps an example--to illustrate how we at the NIH view some of the more general problems of development and application in the biosciences.

Too frequently what seems to be a biomedical problem ideally suited to solution through large-scale development efforts proves--with more careful study--to be something quite different. I would like to illustrate this point with a brief discussion of the major health problem of chronic renal insufficiency--sometimes described as "kidney failure."

Hemodialysis through the use of the artificial kidney is a life-saving measure which can sustain a substantial portion of patients with

complete and chronic kidney failure in nearly normal activity for long periods of time where otherwise they would die. There are, however, technological problems with the equipment now in use. There is danger of infection, hemorrhage, and clot formation for the patient. In addition, this represents a huge economic burden of about $10,000 per year per patient. It is estimated that as many as 5,000 patients each year reach a stage of renal insufficiency that makes them logical candidates for use of such a device. The cost attendant upon the broad use of such a device on a nationwide basis is thus substantial. The amount of professional and technical personnel that would be required is also very large. A partial solution would appear to be simplification, through the development of a do-it-yourself type dialysis unit. This, together with suitable training of family, would do the job at something less than half the cost. Another approach is to develop multiple-patient dialysis equipment devices to improve the utilization of the expensive equipment now required with a consequent lowering of dollar cost per patient. Efforts of these kinds are now underway in a number of places.

It is because of these facts that we undertook a developmental program to simplify both the device and the circumstances of its use some years ago. The extent to which this can be accomplished is not yet clear, except that substantial simplification is feasible.

Meanwhile, dialysis is an essential research tool that can lead to a definition of the specific biological factors that precipitate disability and death in chronic renal insufficiency. Clarification of these factors

will greatly modify the next generation of devices. This objective has been brought closer by the recent demonstration of the effectiveness of diets limited to the minimal amounts of essential amino acids as the total source of nitrogen. Hopefully, this will permit definitive study of the origin and nature of the toxic substances that result in uremia and the consequent development of more specific methods than general dialysis as a means for their removal.

Work in other fields relating to a clarification of the role of tissue antigens in the immunological rejection of organ transplants will lead to more general use of renal transplants, thus lessening the need to resort to hemodialysis as a maintenance technique. This, though, will not be the final answer to the problem of chronic renal insufficiency, since the condition largely results from two major diseases, pyelonephritis, which is repetitive kidney infection, or glomerulonephritis, a chronic disease characterized by acute phases typically leading to complete loss of kidney function. The latter disease tends to attack the fresh renal transplant, destroying the new organ even as it had destroyed the original organ.

So one is driven back to the need to define the cause of glomerulonephritis; to determine the factors concerned with its progression; and hopefully, with such information, to prevent or modify the course of the disease. Here too there is progress to report in the recent development of an animal model system, occurring in a strain of hybrid mice, which for the first time offers substantial opportunities for systematic study of the disease in the laboratory.

The artificial kidney thus illustrates several points: both the complex biological questions as well as the many practical considerations that bear upon the decision to undertake larger scale expensive development programs, also the biological setting required if the overall activity is to be successful.

In conclusion I should like to summarize by emphasizing these fundamental points which embody the essence of what I have just said.

(1) Knowledge of life processes and of phenomena underlying health and disease is still grossly inadequate. In the absence of broad general theory, such as exists in the physical sciences, the development of diagnostic, therapeutic, and preventive capability will continue to be dependent upon empirical approaches, serendipity, and the intuitive brilliance of too limited a number of gifted individuals. Therefore, the hope of major advances lies in sustaining broad and free-ranging inquiry into all aspects of the phenomena of life, limited only by the criteria of excellence, intrinsic scientific importance, and the seriousness and competence of the investigator.

(2) Contrary to general impression, the whole scope of the medical and related sciences is pervaded by a purposeful concern for achieving mastery over the hazards to human life and health. Research in the biomedical sciences contains a high component of practical and problem-oriented activity, though the extent of this portion lacks clear visibility. Unfortunately, research with practical

objectives is too often equated with organized national programs such as cancer chemotherapy, vaccine development, or drug trials. It must be emphasized that such ventures are the exception rather than the rule. In the present state of our knowledge, there are only limited opportunities for highly organized research of a national nature with specific short-range goals. Such ventures must be very carefully selected, for they will be expensive, and, if undertaken in the absence of an adequate scientific base, may be unproductive and hence wasteful of limited resources.

(3) Despite the limitations of the base of biomedical knowledge, progress in our medical capability has been substantial. The benefits of this progress, however, are not universally available. This is evident in the differential between the quality of medicine offered in the Nation's great centers of scientific and academic medicine and in too large a proportion of services at the community level. The solution lies not in medical science but in medical economics and sociology. In this area, the private sector has heretofore been dominant. But the critical nature of these problems has generated growing public concern and public action. If we are to preserve the private character of medicine in this country, the problems must be attacked with greater imagination, concern for the public interest, and willingness of diverse interests to cooperate than has characterized the private scene thus far. The Regional Medical Program of the NIH provides a unique opportunity for such a reformation in medical service.

Finally, Mr. Chairman, I would like to say that I have been very much impressed by the general nature of the questions to which your Committee has directed our attention. These have probed what I would agree are many or most of the key issues in the Federal science policy dialogue underway.

You will recognize, as I do, that where there is more heat than light in these science policy debates, the controversial points have to do with means or priorities much more than with objectives. I think we can all agree that support for the biomedical sciences becomes meaningful in only one context--the effort to push as rapidly as possible toward solution of specific disease problems, and to improve, through health means, the quality of the lives of everyone.

TAB 92

Reprinted from the Summer 1967 issue of the EDUCATIONAL RECORD,
published by the American Council on Education, Washington, D.C.

Thoughts on the Relationships Between Science and Federal Programs

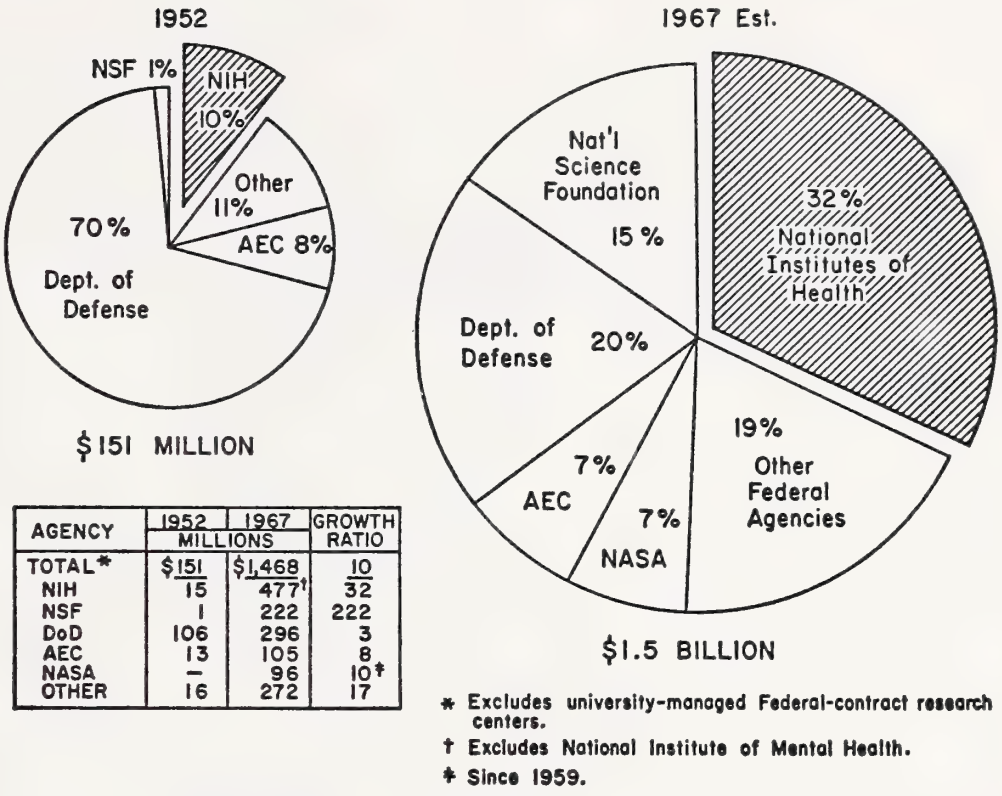
James A. Shannon

THE CHANGING PATTERN of federal involvement in the scientific enterprise and its implications for the universities and the national welfare provoke these reflections on matters broadly affecting medical and health-related research. They arise out of a continuing concern with the problems presented by the growth of academic science, and particularly of biomedical science, between 1950 and 1957.

The facts of growth of research and development (R&D) during the 1950's and '60's are well known, but certain details warrant review. Figure 1 compares the federal support of R&D in institutions of higher learning during 1952 and 1967. By mid-century, World War II dislocations had been largely remedied and a pattern of federal support was established wherein a limited number of mission-oriented agencies predominated. This pattern of support has since changed, bringing the National Institutes of Health, the National Science Foundation, and a number of the non-defense agencies into prominence. The Department of Defense, though tripling in dollars expended, has diminished from 70 to 20 percent of total support. It should be pointed out that these figures exclude the large university-managed federal-contract research centers, such as Los Alamos, Argonne, and the Jet-Propulsion Laboratory. Over the 15 years represented here, industrial research and development has been the major factor of the increase in R&D from about 1 percent to 3 percent of GNP, shown in Figure 2.

Figure 3 illustrates the growth of the biomedical sciences as a proportion of total R&D over the same period. This growth can be viewed as a part of the general expansion of science and technology—a phenomenon of all fields—which reflects the opportunity and promise of social benefit from a more broadly supported R&D activity. The expenditure for medical research has risen only from 6 to 10 percent of all R&D. This proportionate increase is largely a reflection of the small absolute base for these activities in the early fifties.

Meanwhile, there has been a shift in the sources of medical research funds (Figure 4). An increase in those funds from all sectors of the economy was accompanied by differential growth in the federal sector, which



AGENCY	1952 MILLIONS	1967 MILLIONS	GROWTH RATIO
TOTAL*	$151	$1,468	10
NIH	15	477†	32
NSF	1	222	222
DoD	106	296	3
AEC	13	105	8
NASA	—	96	10‡
OTHER	16	272	17

* Excludes university-managed Federal-contract research centers.
† Excludes National Institute of Mental Health.
‡ Since 1959.

Fig. 1: Trends in Federal Support of Research and Development at Colleges and Universities, 1952-67

has exceeded the non-federal since the 1950's. The low expenditures during the immediate postwar years should again be noted.

Growth, when viewed in other terms, is more understandable. I recall two early meetings of the Federation of American Societies for Experimental Biology—the first, hosted by the University of Pennsylvania about 1932, and the second, held in Memphis about 1935. In seeking an estimate of attendance at these meetings, I learned that they were sufficiently small to be handled by committees and that no records were necessary. The Memphis meeting, as I recall, had an attendance of 300 to 350. Contrast this with the 20,815 individuals, including 16,408 scientists, who registered for the annual meeting this April.

Limited opportunities

In the thirties, there were only a few institutions with very vigorous undertakings in biomedical research. Among those were the Rockefeller Institute in New York, the Johnson Foundation in Philadelphia, the Na-

---

REVIEWING THE RELATIONSHIPS *among scientists, academic institutions, and the government, James A. Shannon, retiring Director of the National Institutes of Health, calls for a profound re-examination of the principles and practices that underlie them. In the light of our present national needs and for our future well-being, new patterns seem urgently demanded. Dr. Shannon's paper was originally presented at the 51st meeting of the Federation of American Societies for Experimental Biology in Chicago, April 18, 1967, and will be published in the* Federation Proceedings, *September-October 1967 (26).*

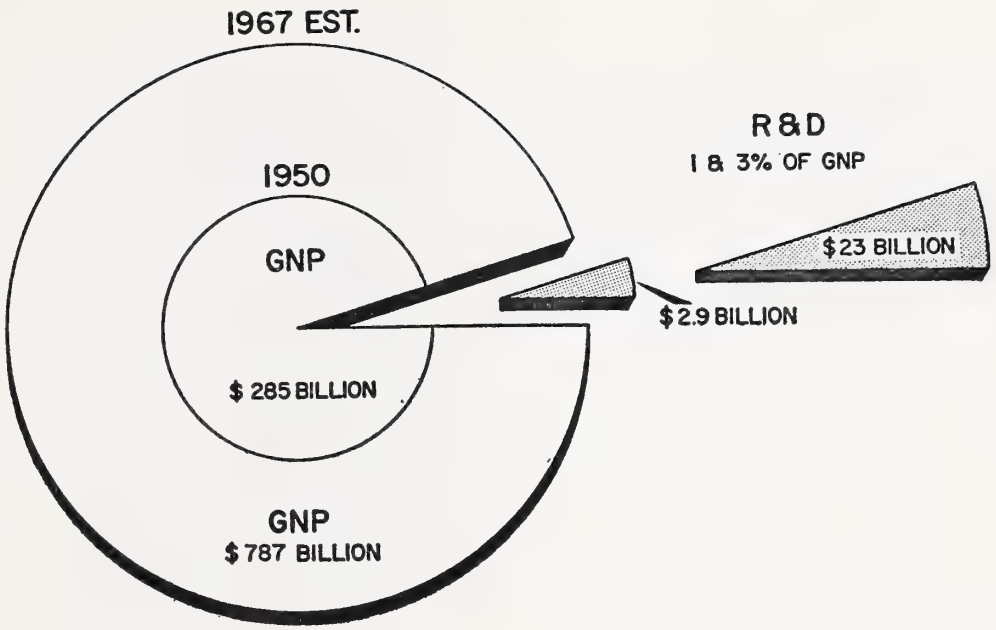


Fig. 2: All Research and Development as a Proportion of Gross National Product, 1950-67

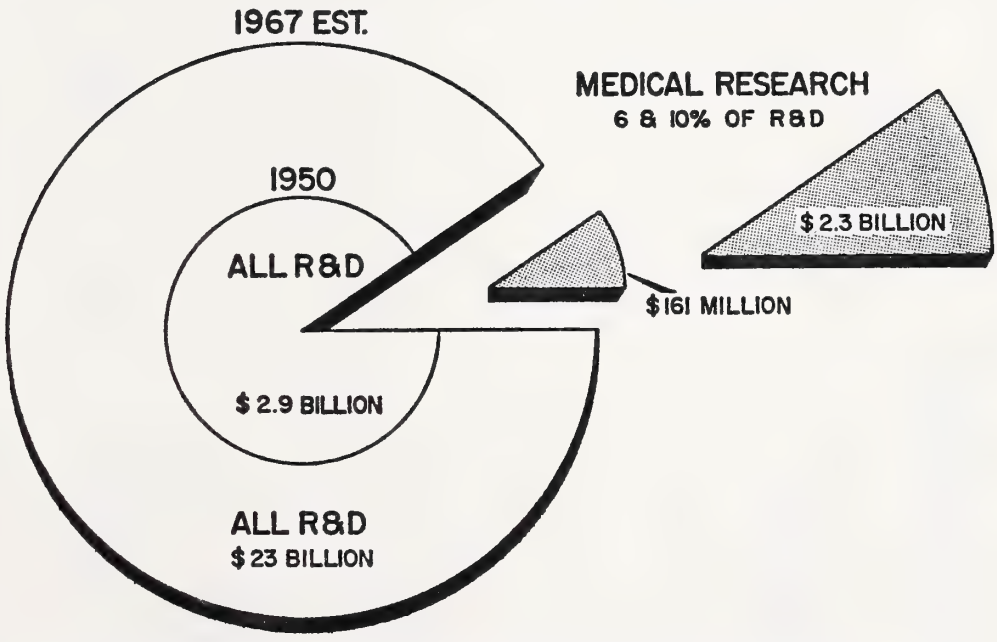


Fig. 3: Medical Research as a Proportion of All Research and Development, 1950-67

tional Institute of Health in Bethesda,[1] the labs of the Carnegie Institute (some of which were biologically oriented), and a limited number of large universities.

While there were many scientific fields with promise, the opportunities for scientists were few. This can be illustrated by a personal survey I made in 1938 and 1939. Anxious about the future of research in physiology, I listed the physiology departments that seemed to offer promise of research capability. There were relatively few, and all those were chaired by professors not due to retire within the next decade. At this time, a career in real research accompanied by true freedom was largely limited to the professor and department head. As a result of these limited prospects, I left physiology and returned to medicine.

This move was made in 1940, purely on the basis of research opportunity. I might add that my own personal research budget at that time amounted to about $4,000, plus my $3,600 salary from the university. This budget permitted the employment of one junior assistant and the purchase of a minimal amount of supplies and equipment. The funds I had, as I recall, were pieced together from grants from the NRC's Committee on Reproduction, the Philosophical Society, and a small organization in New York called the Dazian Foundation for Medical Research. I might also add that each of these grants, though freely spent when once in hand, were obtained on an annual basis as a result of very detailed applications, letter writing, and in one case, prolonged personal negotiations.

Yet my situation was considered to be fortunate. If so, I can only conclude that if research was a serious objective of the institution of which I was a member, then the conduct of science was a very personal thing, arranged for by oneself and supported by the operations of chance—the chance productivity of one's work judged on a year-to-year basis. It was hardly the sort of broadly supported, free-ranging inquiry that is too often

[1] Renamed the National Institutes of Health in 1948.

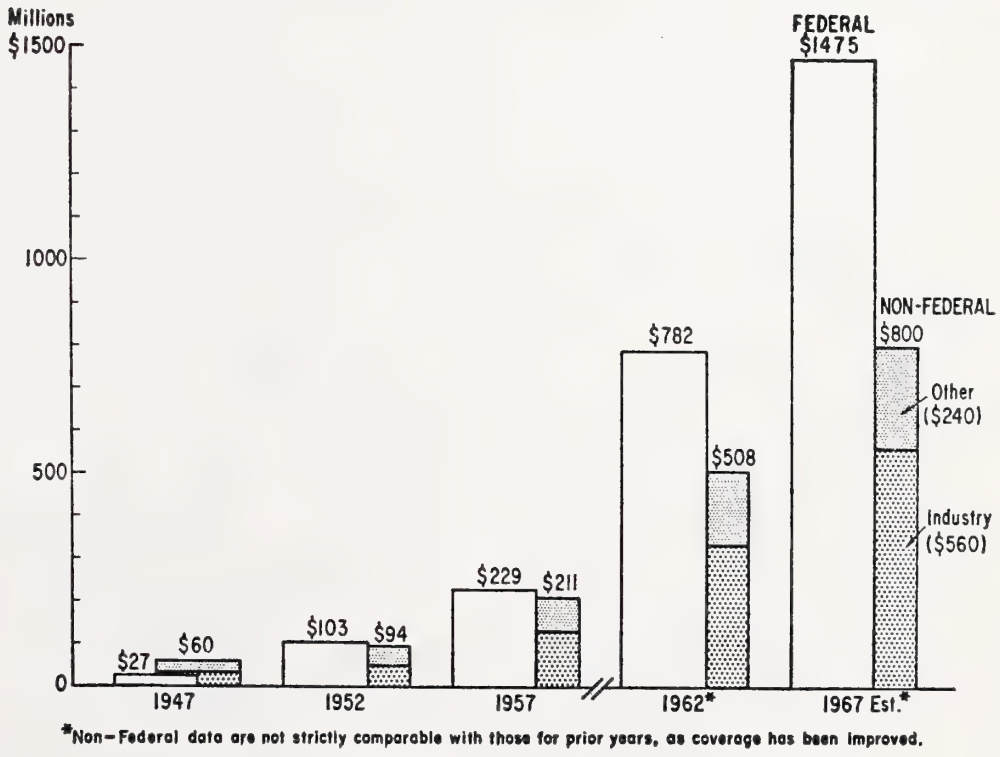


Fig. 4: Sources of Medical Research Funds, 1947-67

ascribed to the past by many mature scientists who should know better.

The price of progress

The situation, as is evident, has changed dramatically. Fairly good opportunities have been secured for our young and developing scientists, as well as for those who are better established. But we have paid a price for this progress. This is manifested in three major ways. First, the continuing instability of the science enterprise resulting from basing the development and maintenance of scientific careers on funds obtained periodically from sources external to the university. Second, a type of accountability that has come to be an inevitable condition of the acceptance of major support from public funds. Third, the inability of the institutions that contain the scientists to determine any longer their own destiny. The federal establishment, the common source of most support of science, has now become a potent force for institutional change.

In the support of medical research, the significance of the federal contribution is again revealed in Figure 5. This compares the total estimated 1967 expenditure for medical research by source and by performer. If one takes into account the fact that industry's funds remain in industry and represent a quarter of the total, the role of the government as a supporter of medical R&D in academic institutions assumes its full import. It should be noted in passing that the National Institutes of Health accounts for 55 percent of the federal expenditure and 36 percent of all funds for health-related research.

Such a group of general relationships, not limited to the biomedical field, gives rise to a situation that might be termed "the plight of academia."

The plight of academia

There are several identifiable root causes. One is the rapid postwar development of R&D, with the emergence of research as a primary responsibility of colleges and universities and the concomitant emergence of a dependence of those institutions upon executive agencies.

Another is the lack of a stable base for an essential enterprise. This results from dominance of the project system in order to ensure excellence of the product and relevance to the mission of the support agency, and from the circumstance of support through

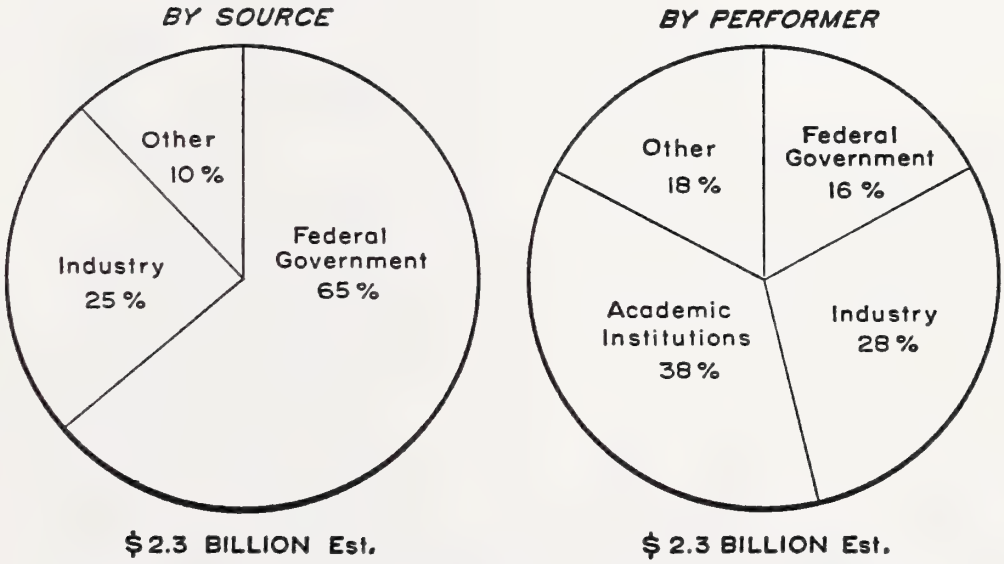


Fig. 5: National Expenditures for Medical and Health-Related Research, 1967

emerging science needs and opportunities or with the number of new scientists available for support. The process of obtaining support through applications for research projects places an increasingly distracting demand upon the investigator, particularly as he attempts to preserve the integrity of his activities through access to multiple sponsors. There is growing demand for project accountability in the use of funds and especially in regard to the investigator's time and effort. There is the underlying tendency for the interests and relationships of the investigator to be divided between his institution and his funding agency, to the disadvantage of the former.

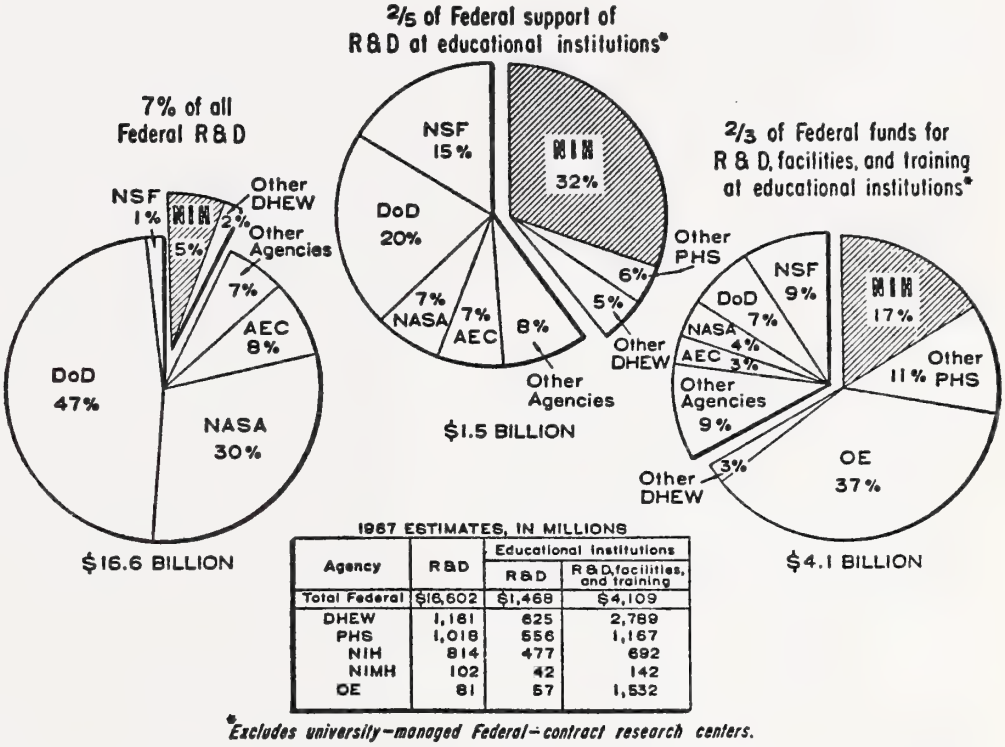
NIH has made efforts in recent years to minimize some aspects of these problems, such as the divisive character of project grants. Thus, we are attempting to devise a system wherein more authority is delegated to the institution for research project management. Nonetheless, the inherent nature of the research project grant will continue to impose sharp limitations upon institutional autonomy.

Institutional worries

From the institution's standpoint, the problems of this postwar growth have taken other forms. Diversity of support, while providing, on the one hand, a broader funding base and protection against the vagaries of any single federal program, has nevertheless meant fundamental and continuing instability in the operating revenues of institutions. The project system with all its many values has diminished the extent to which institutions can exercise full determination over their own destiny in the science areas. Broad support for research as a single function and the limitations of funds for other institutional roles have generated internal distress and imbalance in the full development of institutional programs. The increasing procedural requirements of federal support mechanisms have generated large and distracting administrative concerns. There is no single federal locus of concern with academic institutions *as* institutions in terms of the full range of their function, their important social role, and their developmental needs. Finally, increasing pressures to engage universities with goal-oriented research and with community and other urgent social problems threaten the integrity of their basic institutional character.

This process of growth and current circumstance presents the following problems for national policy and action. Dependence upon diversity has prevented the development of a clear locus of federal concern and policy formulation for academic institutions in terms of the full range of their functions, their important social role, and their developmental needs. The distribution of support functions across various agencies has made it difficult to ensure that federal budgetary decisions provide for the essential basic support and growth of these university functions as a prime consideration of national policy. There are no means to assess the total impact of all federal programs upon universities or to ensure adequate consideration for the preservation of their integrity and the avoidance of distortion. Finally, there are no clear means to deal directly with the problems of institutional development in the broad national interest or to engage with the problems of geographic distribution of the academic resources of the nation.

As a consequence of these circumstances, federal concern for educational institutions takes oblique and limited forms—for example, the memorandum of the President in September 1965, to federal agencies, directing them to administer their support programs to strengthen the educational function of institutions and to provide for their development. Thus, a matter which should be of prime concern and import to the nation, and hence tackled directly, can only be approached in the form of secondary and derivative activities of diverse and largely unrelated federal programs. Such actions, for instance, bring the Department of Defense into a principal role in the process of developing the nation's educational resources. Project THEMIS, a case in point, is an important function under the wrong agency.



1967 ESTIMATES, IN MILLIONS

Agency	R&D	Educational Institutions	
		R&D	R&D, facilities, and training
Total Federal	$16,602	$1,468	$4,109
DHEW	1,181	625	2,789
PHS	1,018	556	1,167
NIH	814	477	692
NIMH	102	42	142
OE	81	57	1,532

*Excludes university-managed Federal-contract research centers.

Fig. 6: Proportions of Support Accounted for by Agencies of the Department of Health, Education, and Welfare

Muddy tinkering

Symptomatic is the fact that the main content of exchange between the government and academic institutions has become the terms of "grant management" and similar procedural matters, with the principal agents of the relationship being administrators and auditors. The particular day-to-day irritations of this process obscure and dominate the underlying issues. As a result, there is a tendency to tinker with the instruments and concepts of the relationship, rather than to modify it in a radical way.

Figure 6 again illustrates the importance of federal agencies in the support of R&D in educational institutions. A paramount role for the Department of Health, Education, and Welfare (DHEW) emerges as one considers the agency's support, first, as a part of all federal funds for R&D; second, for R&D at colleges and universities (also shown in Figure 1); and third, for facilities and training as well as research. The importance of the Office of Education is shown in the third pie, where DHEW is seen to account altogether for more than two-thirds of federal expenditures in institutions of higher education.

It would appear from these considerations that we have reached a point where some major changes must be contemplated in the relationships among the investigator, the academic institution, and the federal government. While the project-support system continues to serve as an objective, equitable, and productive means for advancing specific national objectives, we may have reached the point of maximum utility of that system in the development of a sound science and educational base for the nation, and it may well require broad supplementation by other mechanisms.

Lest I be misunderstood, I should like to emphasize here a vitally important point. I wish to make it absolutely clear that I believe the project system of support as it

has developed over the past 20 years has made an enormous contribution to the advance of science, the development of national programs, and the rise of academic institutions. Also, in the professional schools at least, it has played a direct role in promoting excellence in the educational process. Given the setting and the terms at the time this Nation undertook the development of its science programs, no other mechanism could have served so well, been so productive, or assured the conditions of excellence and relevance in the expansion of our scientific efforts.

Thus, my point here is not to decry the project system as a source of evil, but rather to consider the needs now emerging and the problems of the future course of these vital programs. Without question, project support will continue to play a significant role; but I am exploring the need for additional devices and other arrangements.[1]

In like manner, we may need to re-examine in a contemporary context the values of diversity and variety in the federal sources of support for basic academic functions. This most certainly obtains for the individual scientist. And it may behoove scientists to re-examine their attitudes toward their institutions, and to consider the prospect that their growing direct relationship to federal agencies under the project system may come perilously close to transforming them from scholars to a new type of federal employee, with all the disadvantages and none of the advantages.

We must also re-examine the extent to which the full scope of the nation's scientific and technological needs expressed in federal programs can be met within the present framework and concepts of academic institutions. New academic organizations and new institutional forms may be needed to house, at the very least, parts of the emerging science of the future. We will have to reconsider the establishment of a major federal focus for concern with institutions of higher education in their totality, as well as to consider new arrangements for the exercise of the full federal support functions—construction, operation support, student support, and the function and role of academic institutions in relation to national purposes.

These are issues that must concern us all. They are the broad social and national policy questions which directly and inevitably impinge upon the community of scientists, as faculty of academic institutions, and as citizens concerned with the evolution of sound public policy and programs.

Ripples of distress

Concern within the academic world about these matters is reflected in the reactions to individual irritants and disappointments on the part of administrators of institutions, the scientists themselves, or both. It is reflected in Congress in such situations as the Daddario hearings on NSF, and the Harris hearing on geographic distribution of support and the balance of applied versus fundamental research. It is seen within the executive branch in the directives to the executive agencies from the Office of the President and in the activities of the OST and the Federal Council. It is also reflected in the actions of different support agencies that seek, within their own activities, some way to remedy an admittedly complex and unsatisfactory situation.

I believe that we—the support agencies, the universities, and the public—have drifted into a situation wherein a complex society has developed a broad dependence upon its institutions of higher education for both social and technological advancement. Despite general awareness that these institutions are essential elements for further progress, there has been no general commitment offered by the government that should be concerned with their integrity —and no commitment has been demanded by the institutions themselves.

I am convinced that the future of our nation depends in no small measure on our capability to utilize our intellectual resources in the coming decades, even as our national strength in the first half of the century was largely based on the ingenious exploitation of our natural resources. I am equally convinced that an imperative in determining the intellectual resources available to us will lie

[1] The general problems relating to education *per se* are purposely omitted in this presentation.

in the strength and vigor of our universities.

Moderate tinkering with our support mechanisms will be inadequate to the broad challenge ahead. Rather, what is needed is a radical restructuring of the relationship of the federal establishment to the institutions of higher education. Perhaps some of our most cherished shibboleths, clichés, and traditional postures must be given a hard, realistic reassessment. Among these are: more or less complete dependence upon the project system of support; the value of diverse support through multiple agencies; and the traditional aloofness and disinterest of the scientist in the condition and fate of his institution.

Perhaps we need to consider the values of broader and more comprehensive forms of federal support which can encompass the full scope of academic functions and provide for stability, integrity and sound development of these vital national resources. For the moment, I believe we are in an uneasy state of equilibrium, not satisfactory to any party. Further, this uneasy state must not last too long.

Petty complaints and solutions

As I have noted, there is little evidence of concern with these fundamentals in the academic community at large. Too frequently, the individual scientist seems content to join the small, moderately successful businessman in his day-to-day mix of satisfactions and irritations and his propensities for a moderate degree of obesity and a more than general susceptibility to hypertension and peptic ulcer.

While I appreciate the need to correct the day-to-day irritations that stem from this generally unsatisfactory relationship, I am more concerned with what I perceive to be the short-range and often shallow nature of the complaints. And, particularly, I am concerned with the real lack of planning, on a quite fundamental level, of a new, more productive, and more lasting series of relationships that will satisfy the needs of society for a substantial period. I firmly believe that such planning and exploration are our most urgent needs today. Thus, in a very real sense, I believe we are living in perilous times, but also times of great opportunity, provided that suitable leadership can emerge and we can cut through the distractions and preoccupations of our respective short-range interests and desires.

Federation Proceedings
Vol. 26, No. 5, September-October, 1967
Printed in U.S.A.

Relationships between science and Federal programs

JAMES A. SHANNON
Director, National Institutes of Health, Bethesda, Maryland

It is an honor and a most welcome opportunity to participate in another annual meeting of the Federation. It is my special privilege to be called upon to speak of matters broadly affecting medical and health-related research, before a group that has the capability and indeed the responsibility to contribute in a substantial way to the shaping of the academic setting for science.

I had planned a more ambitious theme—an exploration of "science and social purpose." But as I began to compose some words on the subject, a number of extraneous though related ideas intruded. These arise out of our continued concern with the problems presented by the growth of academic science, and particularly of biomedical science between 1950 and 1957.

FACTS OF GROWTH

The facts of growth of research and development during the 1950's and 1960's are well known, but certain details warrant review. Figure 1 compares the Federal support of research and development (R&D) in institutions of higher learning during 1952 and 1967. By midcentury, World War II dislocations had been largely remedied and a pattern of Federal support was established wherein a limited number of mission-oriented agencies predominated. This pattern of support has since changed, bringing the National Institutes of Health, the National Science Foundation, and a number of the nondefense agencies into prominence. The Department of Defense, though tripling in dollars expended, has diminished from 70 to 20% of total support. It should be pointed out that these figures exclude the large university-managed Federal-contract research centers, such as Los Alamos, Argonne, and the Jet Propulsion Laboratory.

Over the 15 years represented here, industrial research and development has been the major factor of the increase in R&D from about 1% to 3% of gross national product (GNP), shown in Fig. 2.

Figure 3 illustrates the growth of the biomedical sciences as a proportion of total R&D over the same period. This growth can be viewed as a part of the general expansion of science and technology—a phenomenon of all fields—which reflects the opportunity and promise of social benefit from a more broadly supported R&D activity. The expenditure for medical research has risen only from 6 to 10% of all R&D. This proportionate increase is largely a reflection of the small absolute base for these activities in the early fifties.

Meanwhile, there has been a shift in the source of medical research funds (Fig. 4). An increase in those funds from all sectors of the economy was accompanied by differential growth in the Federal sector, which has exceeded the non-Federal since the 1950's. The low expenditures during the immediate postwar years should again be noted.

Growth when viewed in other terms is more understandable. Let me reminisce for a moment. I recall two early Federation meetings—the first, hosted by the University of Pennsylvania about 1932, and the second, held in Memphis about 1935 or 1936. In seeking an estimate of attendance at these meetings, I learned that they were sufficiently small to be handled by committees and that no records were necessary. The Memphis meeting, as I recall, had an attendance of 300–350. Contrast this with the estimated 16,000 scientists who will register this week.[1]

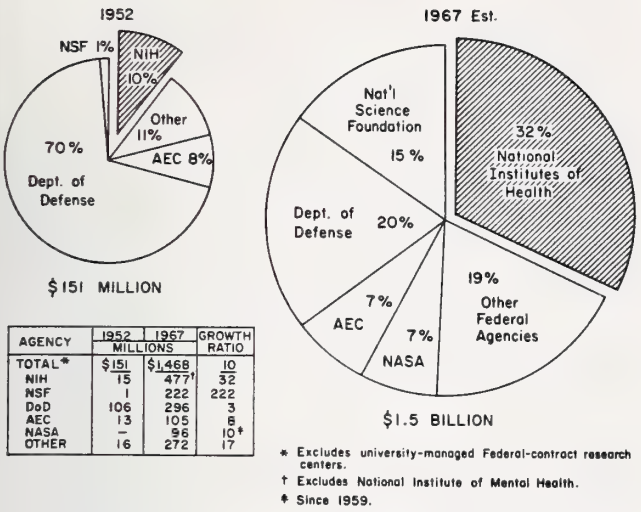
In the thirties there were only a few institutions with very vigorous undertakings in biomedical research. Among those that come to mind are the Rockefeller Institute in New York, the Johnson Foundation in Philadelphia, the National Institute of Health in Bethesda,[2] the labs of the Carnegie Institute (some of which were biologically oriented), and a limited number of large universities.

While there were many scientific fields with promise, the opportunities for scientists were few. This can be illustrated by a personal survey I made in 1938 and 1939. Anxious about the future of research in physiology, I listed the physiology departments that seemed to offer promise of research capability. There were relatively few, and all those were chaired by professors not due to retire

From the Federation General Session on "Biological Sciences: Universities and Government" presented at the 51st Annual Meeting of the Federation of American Societies for Experimental Biology, Chicago, Ill., April 18, 1967.

[1] By actual count, 20, 815 registrants, including 16,408 scientists. (FASEB headquarters, Bethesda, Md., 4/28/67.)

[2] Renamed the National *Institutes* of Health in 1948.



AGENCY	1952 MILLIONS	1967 MILLIONS	GROWTH RATIO
TOTAL*	$151	$1,468	10
NIH	15	477†	32
NSF	1	222	222
DoD	106	296	3
AEC	13	105	8
NASA	–	96	10‡
OTHER	16	272	17

* Excludes university-managed Federal-contract research centers.
† Excludes National Institute of Mental Health.
‡ Since 1959.

FIG. 1. Trends in Federal support of R&D at colleges and universities, 1952–1967.

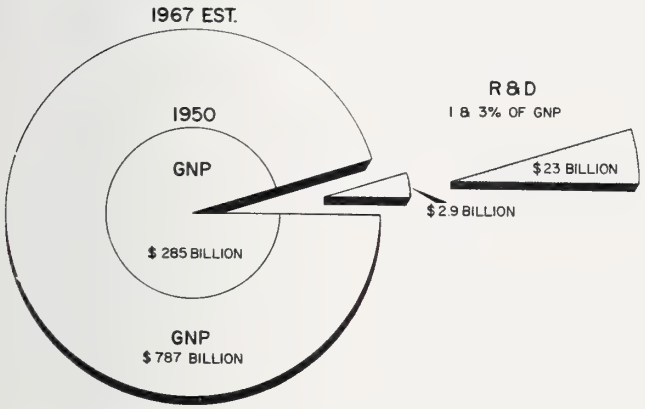


FIG. 2. All research and development as a proportion of gross national product, 1950–1967.

within the next decade, at a time when real research accompanied by true freedom as a career was largely limited to the professor and department head. As a result of these limited prospects, I left physiology and returned to medicine.

This move was in 1940, purely on the basis of research opportunity. I might add that my own personal research budget at that time amounted to about $4,000 plus my $3,600 salary from the university. This budget permitted the employment of one junior assistant and the purchase of a minimal amount of supplies and equipment. The funds I had, as I recall, were pieced together from grants from the NRC's Committee on Reproduction, the Philosophical Society, and a small organization in New York called the Dazian Foundation for Medical Research. I might also add that each of these grants, though freely spent when once in hand, was obtained on an annual basis as a result of very detailed applications, letter writing, and in one case, prolonged personal negotiations.

Yet my situation was considered to be fortunate. If so, I can only conclude that if research was a serious objective of the institution of which I was a member, then the conduct of science was a very personal thing, arranged for by oneself and supported by the operations of chance —the chance productivity of one's work judged on a year-to-year basis. It was hardly the sort of broadly supported, free-ranging inquiry that is too often ascribed to the past by many mature scientists who should know better.

The situation, as you know, has changed dramatically. Fairly good opportunities have been secured for our young and developing scientists, as well as for those better established. But we have paid a price for this progress. This is manifested in three major ways: *1*) the continuing instability of the science enterprise which results from basing the development and maintenance of scientific careers on funds obtained periodically from sources external to the university; *2*) a type of accountability that has come to be an inevitable condition of the acceptance of major support from public funds; *3*) the inability of the institutions that contain the scientists to determine any longer their own destiny. The Federal establishment, the common source of most support of

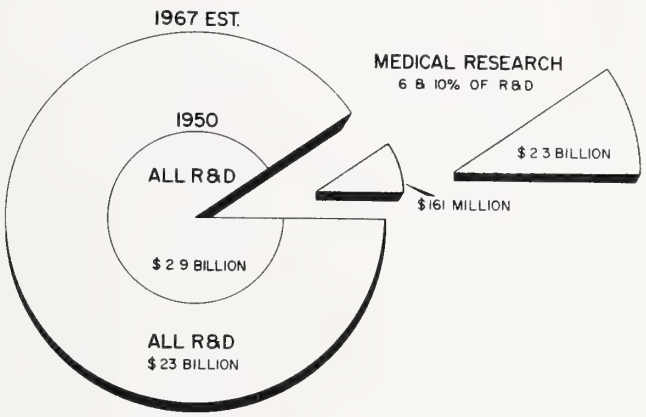


FIG. 3. Medical research as a proportion of all research and development, 1950–1967.

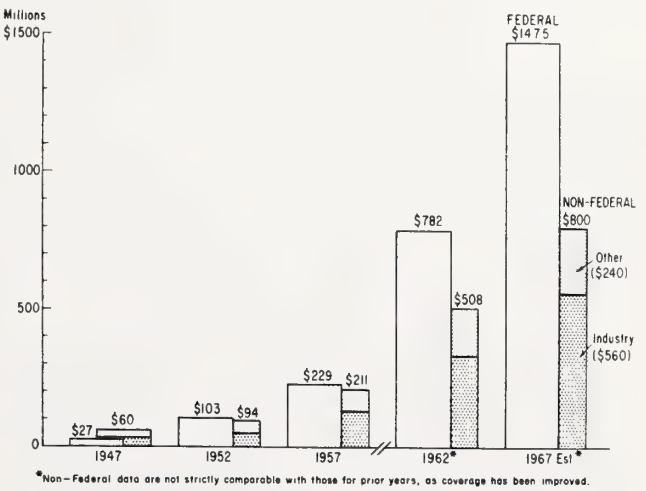


FIG. 4. Sources of medical research funds, 1947–1967.

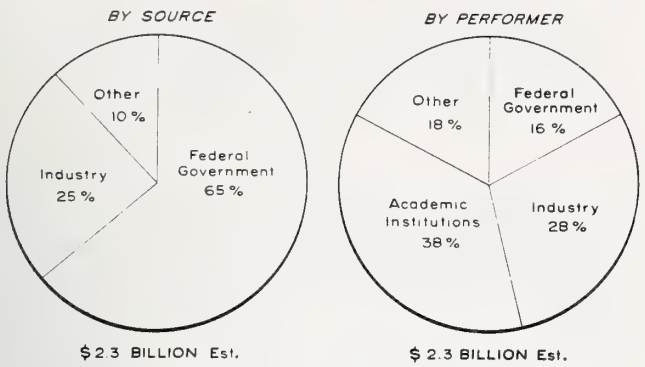


FIG. 5. National expenditures for medical and health-related research, 1967.

science, has now become a potent force for institutional change.

In the support of medical research, the significance of the Federal contribution is again revealed in Fig. 5. This compares the total estimated 1967 expenditure for medical research by source and by performer. If one takes into account the fact that industry's funds remain in industry and represent a quarter of the total, the role of the Federal Government as a supporter of medical R&D in academic institutions assumes its full import. It should be noted in passing that the National Institutes of Health accounts for 55% of the Federal expenditure and 36% of all funds for health-related research.

Such a group of general relationships, not limited to the biomedical field, gives rise to a situation that might be termed "the plight of academia."

THE PLIGHT OF ACADEMIA

There are several identifiable root causes. One is the rapid postwar development of R&D, with the emergence of research as a primary responsibility of colleges and universities and the concomitant emergence of a dependence of those institutions upon executive agencies.

Another is the lack of a stable base for an essential enterprise. This results from dominance of the project system in order to ensure excellence of the product and relevance to the mission of the support agency, and from the circumstance of support through multiple Federal agencies with diverse missions and diverse authorities. Stability is nonetheless a necessity, both to the Federal establishment with its vital social objectives and to the institutions and their staffs.

Also at the root of the problem is the changing role of the university. Once serving almost solely as an information resource, dedicated to the transfer of old knowledge and the development of new, the university is now committed also to the execution of social programs.

In the health field, it is directly involved in the health service system. This role was largely concerned initially—beginning during the second World War—with maternal and child health, and was later extended in a minor way to community mental health. Today there are many such activities, including the burgeoning Regional Medical Programs of NIH and the General Health Care Systems of the Office of Economic Opportunity. In the future, comprehensive health planning at a State level will be an established function affecting the university as well as other official and nonofficial agencies.

To further complicate this situation, there are now voiced a number of other problems of current public concern. One perennial object of criticism is the geographic distribution of Federal support, admittedly uneven because it reflects the unevenness of science capability. A derivative problem in the health field is the tendency of excellence in health services to follow the distribution of excellence in the biomedical-science base for education.

Then, there is worry about the science content of Federal programs. What is a proper balance for support of fundamental versus applied research—of free-ranging research versus managed programs with specific targets? More generally, what should be the Federal commitment to support of broad programs in space, in high-energy physics, in medicine, and the like?

When one views an area of science in a more substantive fashion, little relationship is seen between the structure of the university and the problems, the social objectives, of Federal agencies that support the work in the academic establishment. This is apparent in a mismatch between the departmental structure of the universities and the broad scientific opportunities that have emerged. In the biomedical field it is prominent in the dental, environmental health, developmental and behavioral sciences. It is also evident in more discrete scientific areas such as pharmacology-toxicology.

Similarly, there is a mismatch between the academic structure and the apparatus needed to solve broad social problems. Again in biomedicine, this is seen in the problems of aging, maternal and child health, mental retardation, juvenile delinquency, crime, alcoholism and drug addiction.

Finally, there is a mismatch between the conventional academic structure and developing opportunities in bioengineering and engineering development, particularly those utilizing a systems approach. Biomedical interests include instrument development and automation, partial or complete organ replacement, hospital systems and patient care, and broad social problems with a heavy health overlay.

What are the implications of these problems? The questions and criticisms are reasonable, and just cause for public concern. Cannot the university, which has performed so well up to now, provide the answers?

At the outset, we must recognize that the questions are not simple. They create added problems, the solution of which implies actions that depart radically from prior experience. Moreover, they come at a time when social demands of another sort preclude saturative support of the ongoing academic endeavor. And in any case, there are no general resources available for support of the full scope of university functions.

These matters have serious implications to the scientific investigator, the institution in which he works, and the Federal establishment to which these two relate.

THE PROBLEMS

For the investigator. From the point of view of the academic scientist, the current situation stemming from the postwar growth presents certain critical problems:

1) The rate of funding of academic science through the various Federal programs is not keeping pace with emerging science needs and opportunities or with the number of new scientists available for support.

2) The process of obtaining support through applications for research projects places an increasingly distracting demand upon the investigator, particularly as he attempts to preserve the integrity of his activities through access to multiple sponsors.

3) There is growing demand for project accountability in the use of funds and especially in regard to the investigator's time and effort.

4) There is the underlying tendency for the interests and relationships of the investigator to be divided between his institution and his funding agency, to the disadvantage of the former.

As you know, NIH has made efforts in recent years to minimize some aspects of these problems, such as the divisive character of project grants. Thus we are attempting to devise a system wherein more authority is delegated to the institution for research project management. Nonetheless, the inherent nature of the research project grant will continue to impose sharp limitations upon institutional autonomy.

For the institution. From the institution's standpoint, the problems of this postwar growth have taken other forms.

1) Diversity of support, while providing, on the one hand, a broader funding base and protection against the vagaries of any single Federal program, has nevertheless meant fundamental and continuing instability in the operating revenues of institutions.

2) The project system with all its many values has diminished the extent to which institutions can exercise full determination over their own destiny in the science areas.

3) Broad support for research as a single function and the limitations of funds for other institutional roles have generated internal distress and imbalance in the full development of institutional programs.

4) The increasing procedural requirements of Federal support mechanisms have generated large and distracting administrative concerns. There is no single Federal locus of concern with academic institutions *as* institutions in terms of the full range of their function, their important social role, and their developmental needs.

5) Finally, increasing pressures to engage universities with goal-oriented research and with community and other urgent social problems threaten the integrity of their basic institutional character.

For the Nation. This process of growth and current circumstance presents the following problems for national policy and action.

1) Dependence upon diversity has prevented the development of a clear locus of Federal concern and policy formulation for academic institutions in terms of the full range of their functions, their important social role, and their developmental needs.

2) The distribution of support functions across various agencies has resulted in no means to ensure that Federal budgetary decisions provide for the essential basic support and growth of these university functions as a prime consideration of national policy.

3) There are no means to assess the total impact of all Federal programs upon universities or to ensure adequate consideration for the preservation of their integrity and the avoidance of distortion.

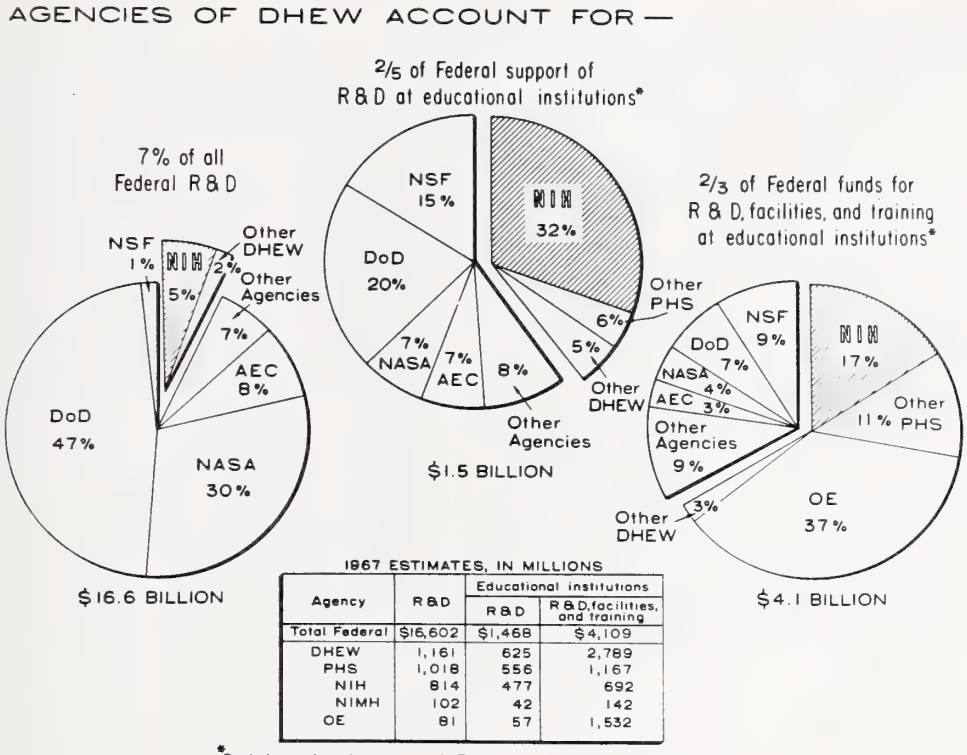
4) Finally, there are no clear means to deal directly with the problems of institutional development in the broad national interest or to engage with the problems of geographic distribution of the academic resources of the Nation.

As a consequence of these circumstances, Federal concern for educational institutions takes oblique and limited forms—for example, the memorandum of the President to Federal agencies of September 1965 directing them to administer their support programs to strengthen the educational function of institutions and to provide for their development. Thus, a matter which should be of prime concern and import to the Nation, and hence tackled directly, can only be approached in the form of secondary and derivative activities of diverse and largely unrelated Federal programs. Such actions, for example, bring the Department of Defense into a principal role in the process of developing the Nation's educational resources. Project THEMIS, a case in point, is an important function under the wrong agency.

Symptomatic is the fact that the main content of exchange between the Federal Government and academic institutions has become the terms of "grant management" and similar procedural matters, with the principal agents of the relationship being administrators and auditors. The particular day-to-day irritations of this process obscure and dominate the underlying issues. As a result, there is a tendency to tinker with the instruments and concepts of the relationship, rather than to modify it in a radical way.

Figure 6 illustrates again the importance of Federal agencies in the support of R&D in educational institutions. A paramount role for the Department of Health, Education, and Welfare emerges as one considers the agency's support, first, as a part of all Federal funds for R&D; second, for R&D at colleges and universities (also shown in Fig. 1); and third, for facilities and training as well as research. The importance of the Office of Education is shown in the third pie, where DHEW is seen to account altogether for more than two-thirds of Federal expenditures in institutions of higher education.

It would appear from these considerations that we have reached a point where some major changes must be



1967 ESTIMATES, IN MILLIONS

Agency	R&D	Educational institutions	
		R&D	R&D, facilities, and training
Total Federal	$16,602	$1,468	$4,109
DHEW	1,161	625	2,789
PHS	1,018	556	1,167
NIH	814	477	692
NIMH	102	42	142
OE	81	57	1,532

*Excludes university-managed Federal-contract research centers.

FIG. 6.

contemplated in the relationships among the investigator, the academic institution, and the Federal Government. While the project-support system continues to serve as an objective, equitable, and productive means for advancing specific national objectives, we may have reached the point of maximum utility of that system in the development of a sound science and educational base for the Nation, and it may well require broad supplementation by other mechanisms.

Lest I be misunderstood, I should like to emphasize here a vitally important point. I wish to make it absolutely clear that I believe the project system of support as it has developed over the past 20 years has made an enormous contribution to the advance of science, the development of national programs, and the rise of academic institutions. Also, in the professional schools at least, it has played a direct role in promoting excellence in the educational process. Given the setting and the terms at the time this Nation undertook the development of its science programs, no other mechanism could have served so well, been so productive, or assured the conditions of excellence and relevance in the expansion of our scientific efforts.

Thus, my point here is not to decry the project system as a source of evils, but rather to consider the needs now emerging and the problems of the future course of these vital programs. Without question, project support will continue to play a significant role; but I am exploring the need for additional devices and other arrangements.[3]

[3] The general problems relating to education per se are purposely omitted in this presentation.

In like manner we may need to reexamine in a contemporary context the values of diversity and variety in the Federal sources of support for basic academic functions. This most certainly obtains for the individual scientist. And it may behoove scientists to reexamine their attitudes toward their institutions, and to consider the prospect that their growing direct relationship to Federal agencies under the project system may come perilously close to transforming them from scholars to a new type of Federal employee, with all the disadvantages and none of the advantages.

We must also reexamine the extent to which the full scope of the Nation's scientific and technological needs expressed in Federal programs can be met within the present framework and concepts of academic institutions. New academic organizations and new institutional forms may be needed to house, at the very least, parts of the emerging science of the future. We will have to reconsider the establishment of a major Federal focus for concern with institutions of higher education in their totality, as well as to consider new arrangements for the exercise of the full Federal support functions—construction, operation support, student support, and the function and role of academic institutions in relation to national purposes.

These are issues that must concern us all. They are the broad social and national policy questions which directly and inevitably impinge upon this community assembled here as scientists, as faculty of academic institutions, and as citizens concerned with the evolution of sound public policy and programs.

CONCLUSION

Concern within the academic world about these matters is reflected in the reactions to individual irritants and disappointments on the part of administrators of institutions, the scientists themselves, or both. It is reflected in Congress by such things as the Daddario hearings on NSF and the Harris hearing on geographic distribution of support and the balance of applied versus fundamental research. It is seen within the executive branch in the directives to the executive agencies from the Office of the President and in the activities of the OST and the Federal Council. It is also reflected in the actions of different support agencies that seek, within their own activities, some way to remedy an admittedly complex and unsatisfactory situation.

I believe that we—the support agencies, the universities, and the public—have drifted into a situation wherein a complex society has developed a broad dependence upon its institutions of higher education for both social and technologic advancement. Despite general awareness that these institutions are essential elements for further progress, there has been no general commitment offered by the Government that should be concerned with their integrity—and no commitment has been demanded by the institutions themselves.

I am convinced that the future of our Nation depends in no small measure on our capability to utilize our intellectual resources in the coming decades, even as our national strength in the first half of the century was largely based on the ingenious exploitation of our natural resources. I am equally convinced that an imperative in determining the intellectual resources available to us will lie in the strength and vigor of our universities.

Moderate tinkering with our support mechanisms will be inadequate to the broad challenge ahead. Rather, what is needed is a radical restructuring of the relationship of the Federal establishment to the institutions of higher education. Perhaps some of our most cherished shibboleths, clichés, and traditional postures must be given a hard, realistic reassessment. Among these are:

More or less complete dependence upon the project system of support.

The value of diverse support through multiple agencies.

The traditional aloofness and disinterest of the scientist in the condition and fate of his institution.

Perhaps we need to consider the values of broader and more comprehensive forms of Federal support which can encompass the full scope of academic functions and provide for stability, integrity and sound development of these vital national resources. For the moment, I believe we are in an uneasy state of equilibrium, not satisfactory to any party. Further, this uneasy state must not last too long.

As I have noted, there is little evidence of concern with these fundamentals in the academic community at large. Too frequently the individual scientist seems content to join the small, moderately successful businessman in his day-to-day mix of satisfactions and irritations and his propensities for a moderate degree of obesity and a more than general susceptibility to hypertension and peptic ulcer.

While I appreciate the need to correct the day-to-day irritations that stem from this generally unsatisfactory relationship, I am more concerned with what I perceive to be the short-range and often shallow nature of the complaints. And particularly I am concerned with the real lack of planning, on a quite fundamental level, of a new, more productive, and more lasting series of relationships that will satisfy the needs of society for a substantial period. I firmly believe that such planning and exploration are our most urgent needs today. Thus, in a very real sense, I believe we are living in perilous times, but also times of great opportunity, provided that suitable leadership can emerge and we can cut through the distractions and preoccupations of our respective short-range interests and desires.

TAB 93

Passano Award Address

Some Comments on the Occasion of the 1967 Passano Award to Dr. Irvine Page

James A. Shannon, MD

It is a great pleasure for me to be with you and to have an opportunity to comment on the accomplishments and talents of Dr. Irvine Page, a distinguished scientist and physician.

You will not be surprised if I talk briefly on some aspects of hypertension and atherosclerosis. Our increasing understanding of these diseases and the scientific contributions of Dr. Page are intimately entwined and interdependent. But I will talk about the diseases in a very general way and only touch lightly upon Dr. Page's role in their exploration.

Three major points might be made: (1) These are important diseases. These and immediately related diseases continue to account for about half of all deaths in the United States. (2) Substantial progress has been made in clarifying both their nature and their management. (3) Finally, our considerations at this meeting can be viewed as a brief pause at a way station in our understanding of these diseases, to which the man we honor has contributed so much.

These two major health problems are, of course, still very much with us today. Hypertension, the most prevalent of the cardiovascular diseases, is present in no less than 17 million adults—more than 15% of the adult population of the United States. On the other hand, the death rate from hypertension has dropped by nearly 50% since 1955. This remarkable reduction stems from the development and use of a battery of effective therapeutic agents that have an ability to lower high blood pressure. Equally important is the growing awareness among physicians that there is no such thing as "benign" hypertension—that even mild hypertension, together with its interplay with atherosclerosis, increases the patient's risk of coronary heart disease and stroke and hence should not be left untreated.

There are grounds for optimism that this downward trend in mortality can continue. But there are no grounds for complacency. Hypertensive disease still claimed nearly 67,000 lives in 1965, and without doubt contributed importantly to many thousands of other deaths from heart attacks and strokes. The drugs in our arsenal are directed at a manifestation rather than at the diseases themselves. We can often control hypertension; in some cases we appear to be able to "cure" it. But we do not understand the underlying nature of these diseases, nor can we prevent hypertension or generally cure it in a definitive manner.

The major differences in where we stand today and where we stood when Dr. Page was beginning a brilliant research career some 40 years ago are these: We now recognize the enormity of the problem. We have committed money, manpower, and resources toward its solution in amounts more in keeping with its importance.

Although a great many people were suffering and dying from hypertension and its complications 40 years ago—how many, we have no way of determining with accuracy—it is safe to say that the number of skilled investigators working on the problem then were few. In fact, the first step in tackling this problem was to recognize that it *was* a disease. The dogma prevailing at the time was that blood pressure is elevated for the purpose of maintaining adequate blood flow through vessels narrowed by arteriosclerosis—that to reduce blood pressure would be to court disaster. It was a comfortable dogma; little could be done about a consequence of growing old.

From the National Institutes of Health, Public Health Service, Washington, DC.

Read on the occasion of the presentation of the Passano Award, Atlantic City, NJ, June 19, 1967.

Reprint requests to National Institutes of Health, Public Health Service, Washington, DC.

Many of you will recall that Dr. Page, while working with Dr. Donald Van Slyke at the Rockefeller Institute, accumulated information permitting the suggestion that high blood pressure might be an abnormal state and that its reduction promised to be beneficial. The problem identified, Page and a few others set about the task of defining the problem and seeking solutions.

Early progress was slow, but these men did not become disheartened. And I doubt that it would have made any difference to them had they known then what we know now—that they were probing a field of extraordinary complexity.

The early work was not marked by spectacular strides or major breakthroughs, but by a slow and steady accumulation of evidence. With the passage of time, this process of accretion resulted in a formidable body of new knowledge about an enigmatic disease. The scientific contributions of Dr. Page to this body of knowledge have been numerous and noteworthy.

Early in his career, Dr. Page sought the isolation of humoral agents with pressor characteristics. This led to the isolation of renal extracts containing what was subsequently identified as the enzyme renin, which can produce an identifiable pressor substance, angiotensin. This was truly a great advance; but to emphasize the paucity of chemical capability at that time, it may be noted that the chemical structure of this compound was not worked out and synthesis accomplished for some two decades (and this by Page, along with Bumpus and Schwarz). However, with angiotensin and later serotonin available for clinical and experimental investigation, a massive body of information was collected in a relatively short time.

It will be most useful if we summarize the progress in our understanding of hypertension in large blocks of information:

1. The first block was the clarification of the chemical basis for the blood pressure as a result of circumstances relating to blood perfusion of the kidney.

2. The second block was composed of two lines of investigation: first, the exploration of newly discovered pressor substances, and second, the interrelation of chemical control of blood pressure to control through neuromechanisms. A highlight along the way was the isolation and subsequent identification of that remarkable substance serotonin.

3. The third block is a synthesis of evidence which presents a working model. This postulates that hypertension, renal or otherwise, is a disease of biological regulation; that the control of normal blood pressure depends upon a complex, carefully fitted mosaic of individual elements both chemical and neural that together in the normal individual are concerned with the proper perfusion of our organs; and that disturbance in the operation of any of a number of the components of the system can result in sustained high blood pressure, or so-called hypertension.

But let us leave the specifics of science and turn to a consideration of some of Page's more general attributes. Some of these stem from the work he has performed, others from attitudes he developed as the driving scientist matured into the scientist-statesman.

The first general attribute relates to his illustration of the basic dynamics of the study of complex human disease. At times this emphasizes the study of the human with or without disease; at other times it emphasizes the experimental animal or discrete systems of an isolated nature; and at still other times it emphasizes the meticulous methodologies of quite fundamental science, removed for substantial periods of time from the more exciting prospects of immediate or even continuing contact with disease.

The second attribute relates to Dr. Page's willingness to devote time and effort, limited resources, to the evolution of sound mechanisms for the adequate support of high-quality science. I have in mind his national service to medicine through his willingness to accept the burden of helping to guide a developing national commitment to the support of research through the use of public funds through mechanisms which were evolving within the administrative apparatus of the National Institutes of Health.

Finally, the third attribute relates to his capable handling of an opportunity to comment on the national medical scene in his editorials for *Modern Medicine*. Here, he has performed most ably, always with sharp professional insight, always with clear exposition of his point of view, frequently with his own brand of wry humor, and when necessary, with devastating critique. This is truly a quality performance.

Having come this far, it is difficult to close, but to go on would be to convert a broad analysis of the contributions of scientist-statesman to a eulogy. Dr. Page fortunately is not ready for such treatment.

I will close simply by saying that one of the very great pleasures of a scientist is to see one of his colleagues acquire stature, as indeed Dr. Page has. Another pleasure is to see his excellence recognized for what it is. This award is a token of such recognition. A final pleasure is to be able to participate actively in honoring a great scientist for his scientific contributions and for his qualities as a person, and I am sure you all join me in these pleasures.

TAB 94

BIOENGINEERING ASPECTS OF MEDICAL RESEARCH PROGRAMS OF THE NATIONAL INSTITUTES OF HEALTH[1/]

James A. Shannon, M.D.,[2/] and Stuart M. Sessoms, M.D.[3/]

On behalf of Dr. Shannon and the many other interested persons at the National Institutes of Health, I will attempt at the outset to convey our deep appreciation for this occasion. It is first an opportunity to define, before an important national group, the NIH position on the field called loosely "biomedical engineering." Second, it is an opportunity to express our sense of gratification at the growing interest of advanced technology in the problems of medicine. Dr. Shannon has asked me to welcome this conference in words paraphrasing the stated purpose of ISA: "Among the major objectives of NIH is the advancement of arts and sciences connected with the theory, design, manufacture and use of instruments in the service of health."

The nature of the interplay between medicine and engineering was strikingly demonstrated a few weeks ago, when an electrical potential generated by the heart of a patient in France was relayed across the Atlantic by a communications satallite. The signals were received at a laboratory of the U.S. Public Health Service in Washington. There a computer produced a detailed analysis of the electrocardiogram, and in less than a minute the information was ready for transmission back to the medical team.

1/ Keynote address at the 22nd annual conference and exhibit of the Instrument Society of America, to be delivered by Dr. Sessoms in Chicago, September 11, 1967.

2/ Director, National Institutes of Health, Public Health Service, U.S. Department of Health, Education, and Welfare.

3/ Deputy Director, National Institutes of Health, Public Health Service, U.S. Department of Health, Education, and Welfare.

This extraordinary achievement has been described as the first such international exchange of diagnostic information. It illustrates the growing importance of the physical sciences and engineering in translating knowledge into tangible health benefits. One could cite innumerable examples of a fruitful relationship extending back to the first medical instruments.

In fact, the interface between medicine and engineering is so extensive as to impose crucial problems of definition. Some attempt to define terms is in order if we are to arrive at a clear view of NIH programs and plans.

The terms "bioengineering" and the slightly more restrictive "biomedical engineering" mean many things to many people. I would advocate that we abandon the terms if there were an adequate substitute to embrace the field under consideration. Let us instead identify the areas encompassed in the so-called biomedical engineering activities of NIH and other constituents of the Department of Health, Education, and Welfare, for these, I believe, represent the full spectrum that the term implies.

Three general areas are involved. The first comprises much but not all of the field called "physical biology." This includes research in which relevant skills derived from the physical sciences, mathematics and engineering are applied to the understanding of simple or complex biological problems. A broader term than "biophysics," though sometimes used synonymously, "physical biology" is reflective of much that composes the leading edge of modern biology on all its fronts.

Some investigations currently in progress would be illustrative:

- Physical and chemical characterization of a species of macromolecule, ranging to the complex membrane phenomena that produce and retain diffusion gradients as a requirement for cellular systems function, or to the coding and release of genetic information and the accidental or purposeful modification of the process.
- Research on biological mechanisms such as the cardio-respiratory apparatus, when viewed as an integrated physical and chemical system with control loops of extraordinary complexity.
- Conduction in the giant axon of the squid, ranging to the formulation of network theories aimed at the evolution of working models for studying the development and operation of the cognitive faculties in man.

The second general area of bioengineering might be termed "engineering development," or as we sometimes say "hardware development." Here, both physical and biological sciences play a major role. But engineering frequently dominates, and the end product is usually visualizable.

The products are of two main types:

- Simulated biological systems.
- Instruments for diagnosis, therapy and research.

A complication of much of this work is that it demands intimate involvement with poorly understood biological mechanisms. This is particularly true of the first category, where much essential knowledge of organ systems is lacking --knowledge that can only come from basic biological investigation. In the field of instrumentation, on the

other hand, there are many opportunities that engineering can exploit directly.

In terms of programing, engineering development is often directed, in contrast with the more free-ranging fundamental research, implying that goals are more surely definable. NIH prefers the contract over the grant for funding engineering development, again reflecting that one can more readily vizualize, specify and predict.

Examples of the first type--simulated biological systems--include:

- Simplification of the present artificial kidney, ranging to the development of devices, if deemed desirable, for replacing vital organs such as the heart or lung.
- Development of adjuncts to defective sensory organs, leading ultimately, one hopes, to mechanical replacement.

Examples of the second type--

- Development of simple sensors to monitor physiological functions without modifying the source, and their extension to complex monitoring systems for use in advanced surgery, in anesthesia, or in the arrest of arrhythmias following heart attack.
- Development of pattern-recognition devices that could simplify cytology.
- Simplification of instruments of measurement, ranging to development of computer-based automated systems for clinical chemistry, for growth and maintenance of tissue cultures, and for a host of laboratory procedures.

- Development of the next generation of electron microscopes, mass spectrometers, and gas chromatographs , all with automated read-out utilizing automatic image intensification or data analysis as needed.

Finally, there is the area that might be called "social engineering"--the application of a combination of social sciences and systems analysis to the description of complex social structures, including those that operate hospitals and deliver medical services, and to the derivation of alternate approaches to provide better and more economical results.

- Development of hospital information systems that would enhance administration and thus service to physician and patient.
- More efficient systems for delivering medical services to the community, such as the Regional Medical Programs for promoting the effective transfer of new knowledge from medical research centers to the universe of practice.
- And going farther afield, one might include the application of systems analysis to problems of the inner city, ranging from highway planning and air pollution control to complex studies of human interaction.

For most members of this audience, the second area--the center portion of the bioengineering spectrum--will be the most relevant. It should be emphasized, however, that medicine and human health are being advanced today through progress on all these fronts, and that NIH is committed to the support of all three. It is also clear that none of the areas has approached its full development or yield.

It has been determined that a high priority shall be given to research along the interface of biology and engineering. The entire spectrum requires more effective exploitation. It should not be supported, however, at the expense of on-going programs of more conventional biology and medicine. More resources are needed if the fields under discussion today are to be developed meaningfully.

The comprehensive interests of the National Institutes of Health in biomedical engineering are reflected in four general categories. The first encompasses engineering in the intramural research programs of the several Institutes through collaboration with the Division of Research Services and through laboratories concerned with technical development. The second category involves the central services provided to the Institutes by the Division of Computer Research and Technology. Third is the extramural program of the National Institute of General Medical Sciences, aimed at the training of scientists, the support of research, and the development of centers of excellence. Fourth is the broad grant program supporting construction and facilities for research, under the Division of Research Facilities and Resources. Finally, there is the social engineering within the aforesaid Regional Medical Programs, devoted to generating efficient systems for augmenting the quality of medicine as applied to heart disease, cancer and stroke.

Scrutiny of these programs leaves no doubt of their efficiency and productivity. Moreover, one sees that little if anything would be

served by consolidating the existing operations within a central structure. The operations are of diverse types, on various levels of organization, and serve manifold functions; and they continue to present rich opportunities where they are.

But if consolidation would be unfeasible and undesirable, certain aspects of these programs should nonetheless be integrated. Changes to that end are in progress. We have learned a great deal about the operation and potential of engineering, particularly in studies of artificial organs, hospital systems, regional and community programs, and field studies using mass data processing. This experience is guiding us in the evolution and transformation of ongoing activities.

Even so, we recognize a lack of machinery for promoting coordination and directed development. It is to this deficiency that current attention is mainly addressed. If maximum use is to be made of engineering in NIH programs, clearly it must have greater visibility and central representation.

It has been decided that engineering development will be represented by a central group. This is to be effected as the entire health function of the Department of Health, Education, and Welfare is studied and modified. The actual structure of the central unit has yet to be determined. It will be organized, however, to act in conjunction with the NIH divisions, and will employ all means at our disposal for the coordination of elements involving engineering and for getting a job done. In summation, engineering development will receive institutional representation, coordination, and support from a central entity with a high degree of advanced engineering capability.

As the central unit develops, there will be derivative changes in NIH programs. Intramural, collaborative and training activities will be affected. Contracts and training grants will be involved. The central unit will offer engineering capability to the Institutes, will draw upon the capabilities of basic scientists and physicians in seeking essential new knowledge, and will act as coordinator of operations in which engineering is paramount. In those Institutes that conduct biological studies requiring engineering, there will be more engineering support.

The essential skills for these activities are now more likely to be found in industry than in the academic world. But the hybrid type of investigator most needed--the biologist-engineer--is scarce in both environments; and there is no way at present of ensuring an adequate supply. New institutional forms in universities will probably be necessary to the propagation of the existing manpower pool. Pilot efforts to this end consist in linkages between schools having the respective capabilities to a high degree

We will foster other such couplings and offer training support to implement their research programs.

Many hybrid subtypes are needed within the general category of biologist-engineer. There will be an increasing demand for neurologists with electronics backgrounds, mechanical engineers skilled in physiology and biochemistry, molecular biologists trained in computer science, psychologists and sociologists versed in systems analysis. Biologists

and engineers can work productively in collaboration, particularly in developmental operations; but the combination of skills in one individual offers advantages in fundamental, creative investigation.

Among the 6,000-or-so professional, subprofessional and technical workers at NIH, there has been a rich preliminary experience in bio-engineering. I would say, however, that sufficient insight and sophistication are lacking for a program of maximum potential. Indeed, the capability necessary to organize and administer such a program is not present anywhere. Facing up to these facts, we have sought information and advice from authoritative sources, such as the National Academy of Sciences and the new National Academy of Engineering.

Our turning to NAE grew out of an appreciation of opportunities in bioengineering on the part of the House of Representatives' Appropriations Committee. In their report of last year on allocation of funds for NIH, a section on "Medical Engineering Development" recommended "further evolution of the collaborative research and development programs of NIH that relate to biomedical engineering and technology."

The report further stated that "it would be a foolish diversion of effort to do so at the expense of the on-going research programs" and that "engineering development programs should be regarded as a distinct activity for which separate budgetary provision should be made."

As a first step, the committee added $250,000 to the NIH budget for the necessary planning, consultation and contractual assistance to launch the effort.

In response to this directive from the Congress, NIH has just negotiated a contract with the National Academy of Engineering for a study of engineering in medicine and biology. The Academy will establish a committee on the interplay of engineering with biology and medicine, to explore the applicability of current engineering theory and practice to biomedical problems and to serve NIH in an advisory capacity.

The committee will examine the future role of engineering concepts and technology in regard to--

- Advancing the understanding of biological systems.
- Developing research instrumentation, diagnostic and therapeutic devices, and artificial organs.
- Designing and developing large-scale systems encompassing major social functions such as health services, as well as their sub-units such as hospitals.

The NAE committee will also examine the question of how to convert the ill-defined region that lies between engineering and biomedicine into recognized fields of activity and to develop prototype institutions in this field. The contract provides funds for subcontracting a detailed examination of at least three institutions, or institutional complexes, involved in bioengineering activities.

Another NIH approach to the question of organizing for maximum use of engineering has been a preliminary study by Aerospace. This has helped to define a series of moves, but not the entire attack along the whole front. It is hoped that these various approaches, supplementing NIH experience, will soon point to an inspired combination of structure and action.

The current level of funding for biomedical engineering activities conducted or supported by NIH is about $35 million a year. We know that this is inadequate. While an exact figure cannot be assigned as yet, we are convinced that growth in this area should be geometric for the next several years. There should be at least an annual doubling of funds for three or four years--$35 million, $70 million, $140 million and so on. Part of this would support natural growth in the existing programs. New projects would be selected for development, within the existing structure as well as the central group. The latter entity would receive the bulk of the new funds. And in all areas, adequate attention would be given to the balance of projects, training, and resources.

Meanwhile progress across the board should be assessed and a more refined growth curve established.

It is our earnest hope and expectation, then, to create at the National Institutes of Health a new central organization dedicated to the coordination, conduct and support of biomedical engineering along

a wide spectrum of operations. Its mission would be to administer a program of excellence, to serve as a resource, and to concern itself with the broader interests of the field. In this purpose, there is ample endorsement by the Congress, the Department, and interested nonfederal groups. Within NIH, there is considerable enthusiasm over the prospects of launching such an endeavor.

I regret the need for vagueness at this time concerning the exact form of the organization, its staffing, scope and budget. These determinations must await the facts that will help to define the function. One can assuredly predict, however, that a primary role of this body will be to promote to a new order of magnitude the already prolific union of instrumentation and the life sciences.

TAB 95

ACADEMIC SCIENCE AND THE FEDERAL ESTABLISHMENT*

James A. Shannon, M. D.**

Today I shall confine my remarks to the universe of the medical school, excluding the considerable portion of academic science that is conducted in other institutions.

The primary mission of the National Institutes of Health is the development of new knowledge in the many areas of science and medicine that may be relevant to the solution of serious disease problems. The importance, the magnitude, and the nature of this mission is such that certain derivative functions emerge as essential. Some of these relate to a quantitative and qualitative concern for the manpower requirements of the research objective. Others relate to the health and vigor of the institutions wherein much of the new knowledge is gained. The institutions concerned with medical education are of special interest in this regard because of the key role they play, and must continue to play, in the overall endeavor. Finally, the new knowledge derived from the effort must be brought to the point of applicability if the program is to be successful. In relation to this function, the medical school has a key role as the primary interface

---

*Presented at the University of Maryland School of Medicine, Baltimore, Maryland, November 21, 1967.

**Director, National Institutes of Health, Public Health Service, U.S. Department of Health, Education, and Welfare.

between the development of new knowledge and its general use within the community at large.

It is for these reasons that the National Institutes of Health has traditionally concerned itself with the viability of medical schools and the impact of its programs upon them. Indeed, our involvement with this segment of higher education is such that at times the medical schools have viewed the functions of NIH and their own as coterminous. This is not the case, but the involvement is truly intimate; and should medical education become a statutory responsibility of NIH, primary in nature, then the involvement will be even greater.

Today I should like to consider certain aspects of medical schools (or university medical centers) as they appear to an interested observer. Then I shall touch briefly on a number of Federal programs that increasingly affect medical school activities--programs with implications relative to the organization of these complex enterprises and their functional adaptation in pursuit of their missions. Perhaps the simplest way into this array of considerations is to examine the evolution of the medical school over the past quarter century, from a simple training facility to the modern medical center with its manifold functions.

We might begin with the early phase extending, say, from 1912, when sweeping reforms, sparked by Flexner's survey and report, were first effected--i.e., primarily during the 1920's--to America's entry into World War II. The tremendous changes wrought in our social and economic life by World War I, its aftermath during the '20's, the Great Depression, and the New Deal did little in themselves to alter the organization and

functions of the evolving medical school. Rather, they merely modulated the development through modification of the available financial resources.

In 1940, one still found an institution dedicated to the education of physicians and engaged only secondarily in science and the rendering of services. Science, generally viewed by then as vital to medical education, was largely undifferentiated and pursued in the course of teaching. Services were provided almost solely in affiliated hospitals, where residency training programs were generally established but on a modest scale. The clinical services, again, subserved the educational function or the performance of a small-scale research activity. It is true that in the major metropolitan areas, large municipal hospitals were/staffed professionally and operated by medical schools; but generally speaking, the activity was accepted as a compromise with the ideal--a compromise designed to provide clinical material for institutional purposes.

The World War II years 1941-46, so disruptive to civilian activities and institutions, can be viewed as a pause in the evolution of medical schools. They were to be radically altered in the postwar years in response to a number of specific factors:

- The emergence of devastating chronic illnesses as prime targets for medical activity of diverse character.
- The massive move of the medical profession toward specialization, and the formal training required for board certification.
- The revolutionary force of medical research and the eager pursuit of new knowledge, with accelerating public support.

During and right after World War II, the productivity of medical research was dramatically demonstrated and the public's interest aroused. Among the more significant and striking advances were the antimalarial compounds; the antimicrobial agents--namely, the sulfonamides and such antibiotics as penicillin and streptomycin; advances in trauma, shock, and blood substitutes; and extensions of surgical practice. Another powerful incentive to public support of science was of course the dramatic termination of the war with the positive use of atomic energy.

It is interesting that the bomb, which the public linked with cancer through the property of radioactivity, itself engendered a demand for medical research on an unprecedented scale. The major thrust here was the anticipated productivity of managed, target research, rather than the specific implications of nuclear medicine. (Medical studies were undertaken at Brookhaven as an afterthought in 1948-49.) One might add that while the general linkage of the atomic bomb with medical-research potential may have been illogical, the use of radioactivity in metabolic tracers has more than justified the public confidence. The bomb and subsequent tangible products of the mind, such as new drugs, computers, and spaceships, have established intellectual capability as the definitive measure of a society's strength. And science has become widely accepted as an integral part of our culture.

The two decades from 1946 to 1966 saw biomedical science isolated for selective Federal aid. Between 1940 and 1946, the national expenditure for medical research merely doubled. The subsequent increase, led by Federal action, has been about 25-fold. Yet this support has

been largely confined to the individual research project. Medical research has been an enterprise sustained on a piecework basis. But during the past decade the Congress has recognized in a quite specific manner a number of parallel program needs. These include the need for--

- Extensive training of manpower.
- Construction of research facilities.
- Expansion of special resources, such as animal colonies and clinical research centers.
- General support of research and research training through institutional grants.

Hence, this has been a decade of growth in medical research potential, mainly through the mission-oriented, project-focused support of NIH. Altogether, the Federal Government now supports about two-thirds of the Nation's medical research, with NIH providing 55 percent of the Federal total.

As I have emphasized elsewhere, the individual project system has been, and continues to be, a productive and efficient means of financing research; but it has led to strain in the administration of schools. Not until 1963, with the passage of the Health Professions Education Assistance Act, did the Congress recognize financial needs of the medical school itself and the medical student (though grants for the training of nurses and public health workers were instituted in 1956). Today, there is an emerging recognition that the vigor and productivity of medical education as well as research are social responsibilities--that the Government must commit itself more broadly to medical education and the effective operation of the schools and centers that provide it.

The evolution of the medical school, reflecting its role in the history I have outlined, can be diagramed. The first chart represents the medical school in 1940. Science and clinical service, as I have mentioned, were performed in a limited way as essential aspects of physician education. The advancement of medical knowledge in this setting depended almost solely on undifferentiated research. Together, these functions--science, service, and instruction--were mutually reinforcing.

Chart 2 illustrates some additional responsibilities assumed during formative postwar years:

- Graduate education to yield Ph.D.'s in biomedical sciences, M.D.'s with postdoctoral training in science and the medical specialties, and opportunities for the continuing education of physicians. During this period, the schools also greatly expanded their programs of residency training.
- Education of nurses and other auxiliary professionals and technologists was extensively undertaken.

To foster many of these additional functions of the schools, the Federal Establishment has given broad support.

Chart 3 brings us to the modern university medical center, in which still other functions are added to the educational responsibilities outlined above. These include--

- Organized research, conducted largely in response to the national aims and objectives. The organizational units range from complex but medium-sized departmental programs to full-blown research institutes.

been largely confined to the individual research project. Medical research has been an enterprise sustained on a piecework basis. But during the past decade the Congress has recognized in a quite specific manner a number of parallel program needs. These include the need for--

- Extensive training of manpower.
- Construction of research facilities.
- Expansion of special resources, such as animal colonies and clinical research centers.
- General support of research and research training through institutional grants.

Hence, this has been a decade of growth in medical research potential, mainly through the mission-oriented, project-focused support of NIH. Altogether, the Federal Government now supports about two-thirds of the Nation's medical research, with NIH providing 55 percent of the Federal total.

As I have emphasized elsewhere, the individual project system has been, and continues to be, a productive and efficient means of financing research; but it has led to strain in the administration of schools. Not until 1963, with the passage of the Health Professions Education Assistance Act, did the Congress recognize financial needs of the medical school itself and the medical student (though grants for the training of nurses and public health workers were instituted in 1956). Today, there is an emerging recognition that the vigor and productivity of medical education as well as research are social responsibilities--that the Government must commit itself more broadly to medical education and the effective operation of the schools and centers that provide it.

The evolution of the medical school, reflecting its role in the history I have outlined, can be diagramed. The first chart represents the medical school in 1940. Science and clinical service, as I have mentioned, were performed in a limited way as essential aspects of physician education. The advancement of medical knowledge in this setting depended almost solely on undifferentiated research. Together, these functions--science, service, and instruction--were mutually reinforcing.

Chart 2 illustrates some additional responsibilities assumed during formative postwar years:

- Graduate education to yield Ph.D.'s in biomedical sciences, M.D.'s with postdoctoral training in science and the medical specialties, and opportunities for the continuing education of physicians. During this period, the schools also greatly expanded their programs of residency training.
- Education of nurses and other auxiliary professionals and technologists was extensively undertaken.

To foster many of these additional functions of the schools, the Federal Establishment has given broad support.

Chart 3 brings us to the modern university medical center, in which still other functions are added to the educational responsibilities outlined above. These include--

- Organized research, conducted largely in response to the national aims and objectives. The organizational units range from complex but medium-sized departmental programs to full-blown research institutes.

- Community programs, largely goal-oriented and peripheral to what has been historically the primary function of the medical school or center. I have in mind the extensive programs in mental health and mental retardation, the community health activities of the Office of Economic Opportunity, and the involvement of the medical faculty with the operations of the Regional Medical Programs, Comprehensive Health Planning program, and the Center for Health Services Research.

Broad Federal support has not been lacking in the areas of high public interest, and indeed has encouraged the schools to extend themselves in diverse and diverting ways. Despite serious efforts on the part of the Federal Establishment to help the schools meet overhead and administrative costs of research, and to foster constructive over-all measures in support of science and social services, many schools have encountered administrative difficulties more or less related to the explosion of secondary programs. Quite valid concern for these important programs has focused attention on the fundamental instability of the present framework for support of the academic enterprise. One is struck by the singular equation, The better the superstructure, the weaker the building. And above all, the unique function of the medical school--the education of physicians--has become submerged.

The next chart (4) superposes a dotted-line frame over the previous diagram, isolating the functions essential to medical education. In an abstract way these might be called the "core" activities of the medical school. Certain administrative entities are also shown: the dean and his staff, the heads of research departments and community programs.

Evolving offices concerned with directing programs of graduate education and ancillary training are sketched in. That the core activities are not supported in a purposeful and adequate way has, in my view, contributed to the difficulties I have mentioned, and in fact jeopardizes the entire structure.

Let me dilate on this point, which is the burden of my remarks. The core function of the medical school--namely, physician education and such activities as render the physician maximally effective, including some research, service, and graduate education--can only be adequately discharged if well defined, stably financed, and capably directed. Any outside support of this function (and all would agree that some is needed) must derive from authorities and programs reflecting order and definition on the part of the schools. It is important, then, to define, for administrative and perhaps for accounting purposes, just where this dotted frame falls--what it encompasses and what it excludes. More specifically, we must define the core's program, staffing, tangible assets and liabilities, fiscal resources, and results.

Yet this operation must not be divisive in the sense that it subvert the vital interplay between core and peripheral activities, which constitute an essential extension of the social and science base for development of the graduate student. I am referring primarily to administrative distinctions. On the other hand, I do not have in mind a mere exercise to satisfy government auditors. I believe that identification of the core of the medical school can lead to better programing, direct and impartial support, and strengthening of all the school's activities.

There are two possible approaches to redressing the imbalances occasioned by Federal support of medical research and services. One is to curtail the funds going for individual projects and expand general support to the institution. The other is to recognize that funds now available are subject to limited redistribution if adequate additional funds are forthcoming. The first alternative is rejected as neither realistic nor socially advisable. But these meritorious projects are indeed related to a total organization--one having a strong core which is both capable of balanced growth and can be made resistent to parasitism from the superstructure; and it is the vitality of the total organization that concerns us in 1967. The present vagueness as to the proper relation of the medical school with its multiple functions to the Federal Establishment must be clarified if this essential relationship is not to threaten the very future of medicine.

If the medical schools are willing to review their activities along the lines proposed, I venture to say that the Federal Establishment will respond with appropriate action. The types of program already receiving support might be reviewed briefly:

- Organized research programs--fundamental, applied, and developmental. This is the area of the greatest potential for growth over the next decade.

- Undifferentiated research under the research project system, fellowships, and training grants. Such research reflects the scientific community's judgment of what is important, and must remain the keystone of NIH activity.

- Resource programs, providing for construction of research facilities, general research and training support on a formula basis, and project grants for science development.

- Educational programs, including construction projects, basic improvement grants, and project development grants. The present program constitutes a base for necessary extension and modification which, with suitable support mechanisms and adequate funding, can provide, in conjunction with the general research support programs, a sound base for total institutional development.

- Programs that involve community activities with social purposes, such as those I have named.

So there exists an impressive array of Federal programs serving the direct operations of the medical school. The bulk of these are up for renewal by the Congress. They should be of deep concern to all who are engaged in planning for medical education and those who wish to expand their programs beyond education per se.

In addition, broad support of the core activities can emerge as an important part of the new package, but this depends primarily on the thoughtfulness and leadership derived from the schools themselves. It is difficult to define general needs. Each school is different, and the program in each case is a matter for internal judgment. Yet, no school is altogether unique, and their similarities outweigh their differences. In other words, each school has a central set of activities subject to some definition, to some accounting, and to some administration, and dependent to the extent required upon the Federal Establishment.

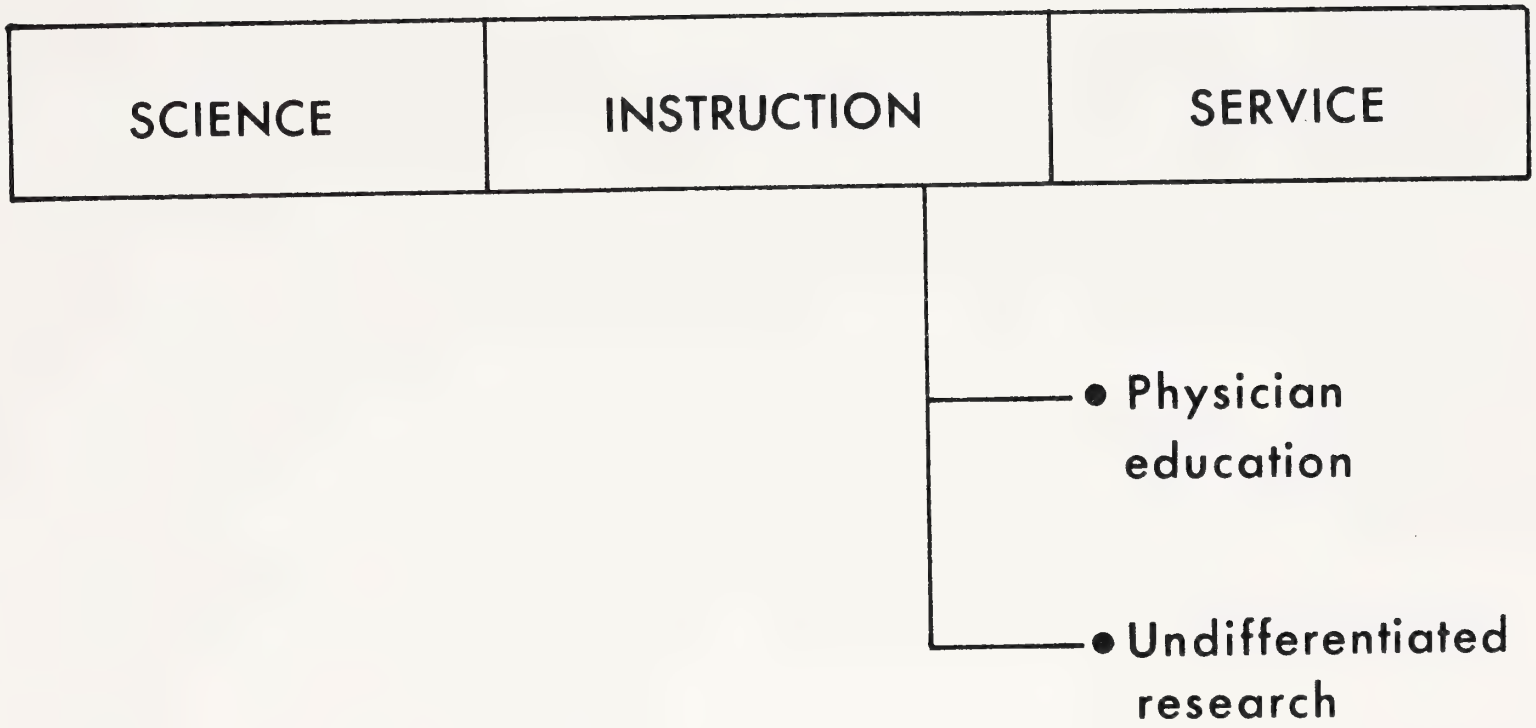
Assuming that appropriate authority can be obtained and properly implemented with adequate support, the schools will become an increasingly important national resource for personnel--researchers, teachers, administrators--and will thus be better prepared to satisfy a growing social need.

But the schools themselves must take the lead in defining their objectives. Once they have peeled away the nonessentials, and have stabilized the essentials in a suitable setting, then educational innovations will be possible. Again, I believe that medical education can look to the Federal Establishment for subsidy. But the schools must be prepared to offer wisdom and counsel as to how this, as only one of several competing major national objectives, can best be carried forward.

CHART 1

THE MEDICAL SCHOOL

1940



Assuming that appropriate authority can be obtained and properly implemented with adequate support, the schools will become an increasingly important national resource for personnel--researchers, teachers, administrators--and will thus be better prepared to satisfy a growing social need.

But the schools themselves must take the lead in defining their objectives. Once they have peeled away the nonessentials, and have stabilized the essentials in a suitable setting, then educational innovations will be possible. Again, I believe that medical education can look to the Federal Establishment for subsidy. But the schools must be prepared to offer wisdom and counsel as to how this, as only one of several competing major national objectives, can best be carried forward.

CHART 1

THE MEDICAL SCHOOL

1940

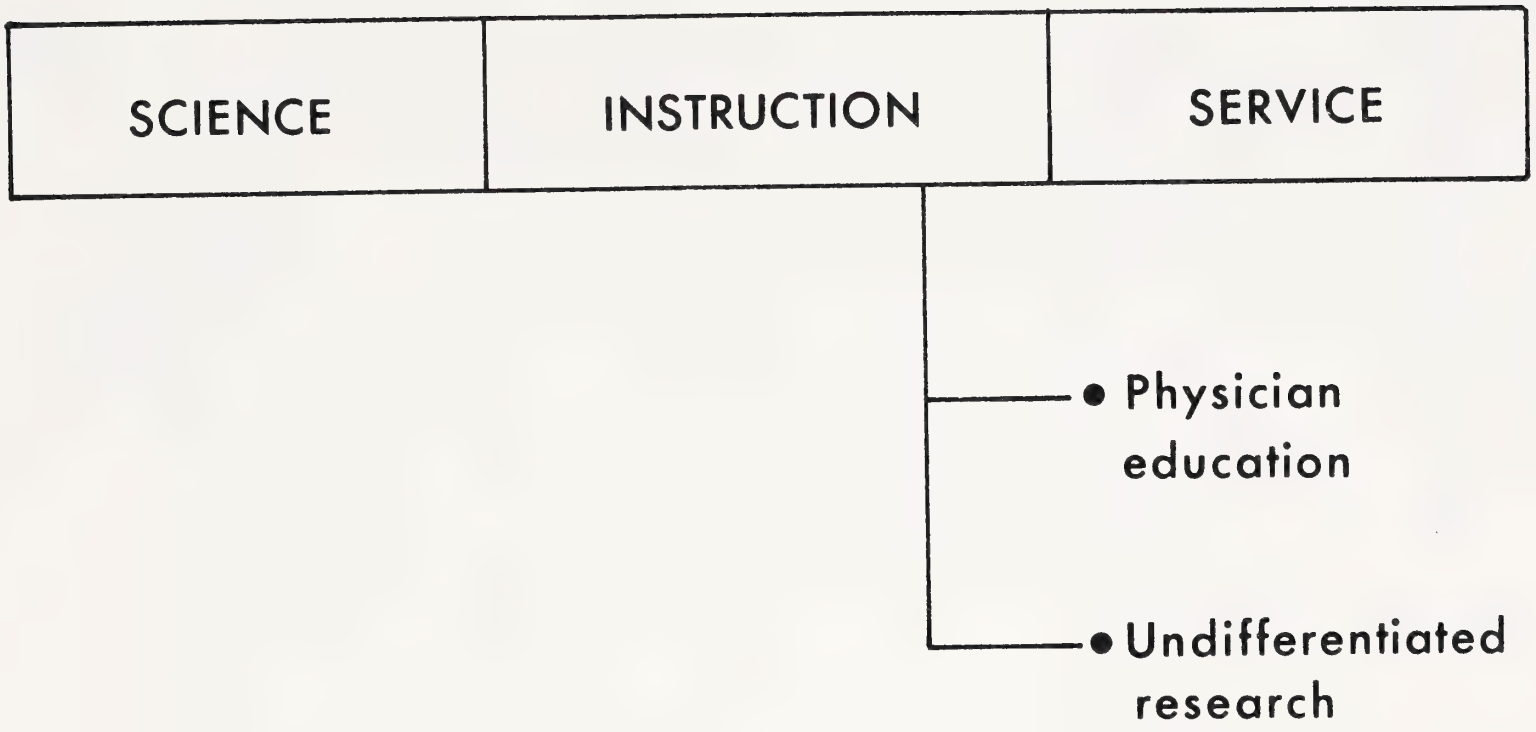
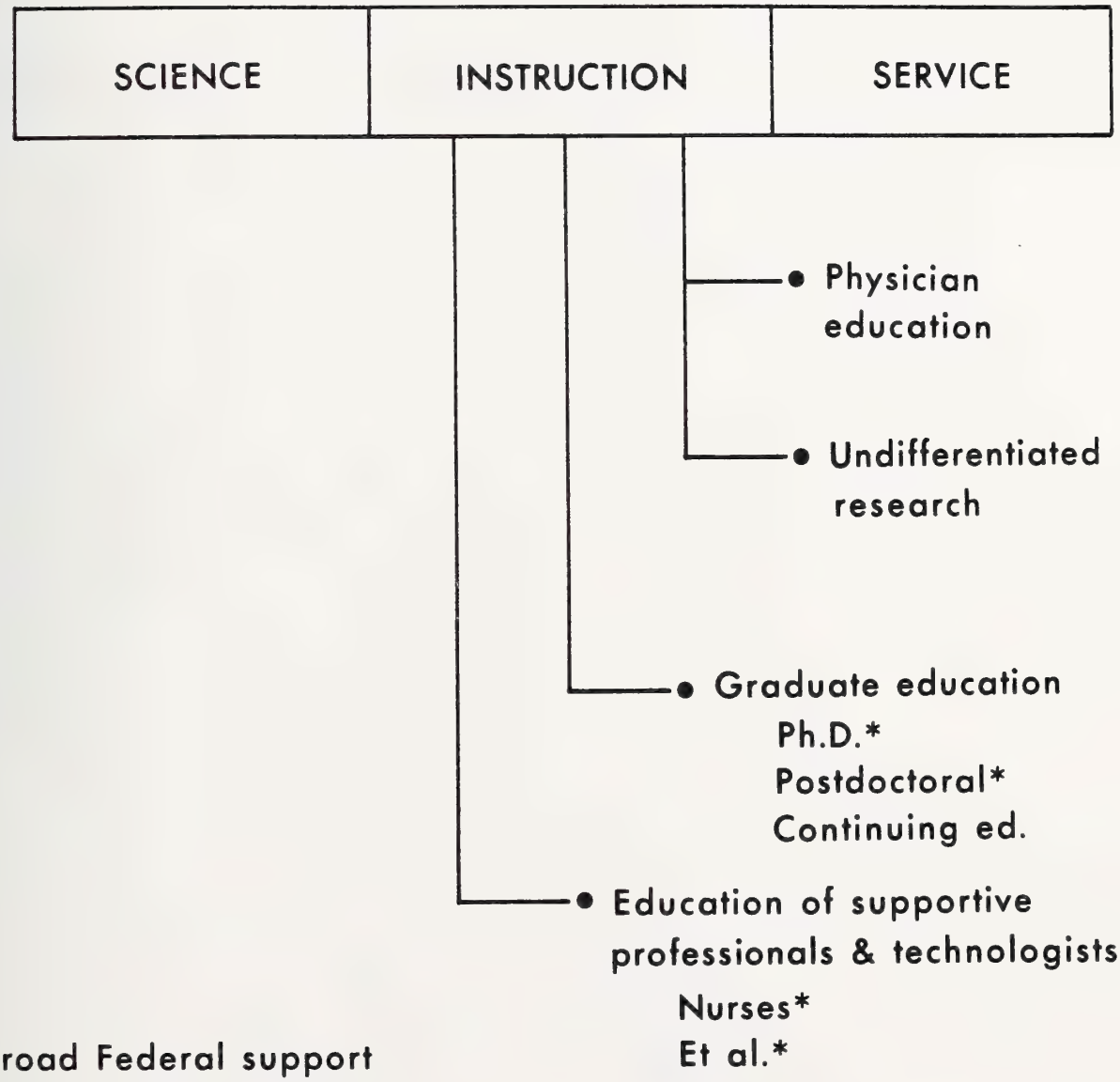


CHART 2

THE MEDICAL SCHOOL

Developing Educational Functions

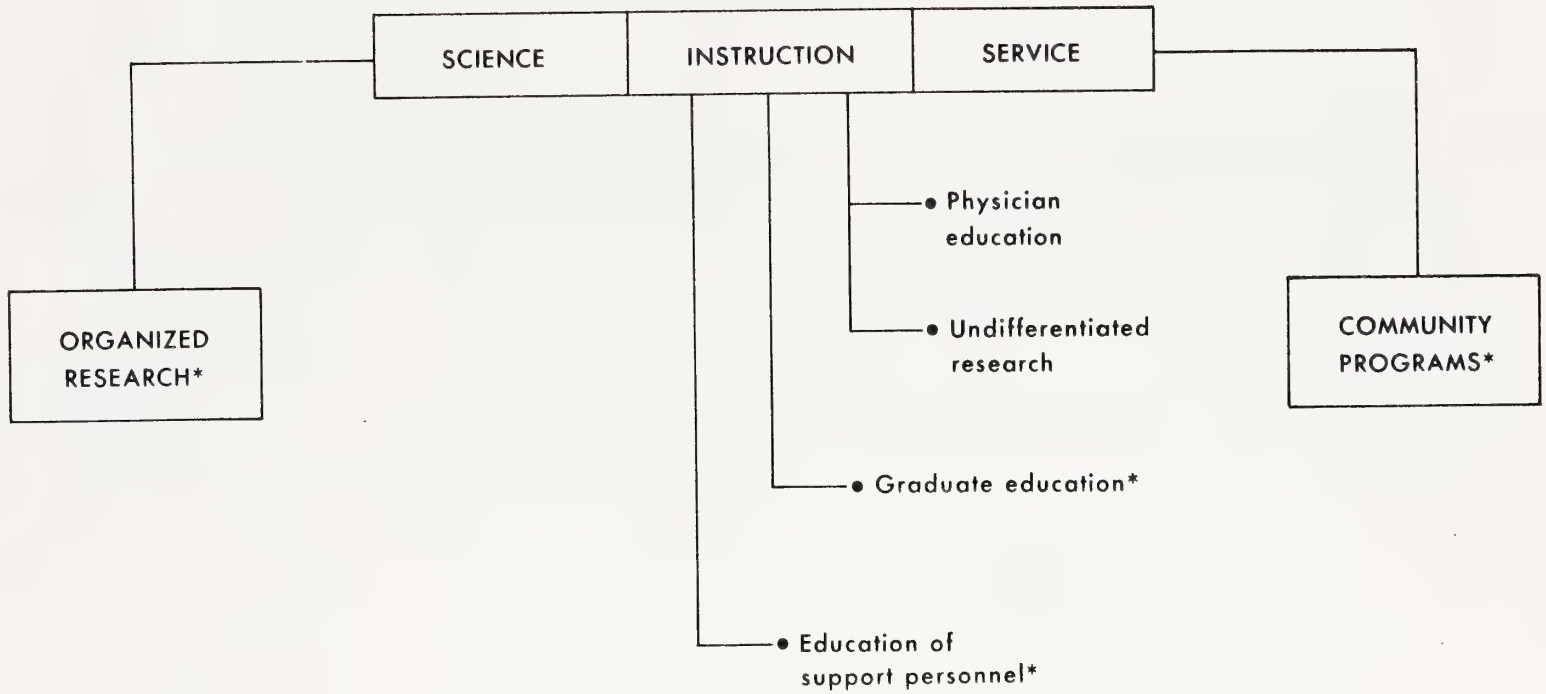


*Broad Federal support

CHART 3

THE MODERN UNIVERSITY MEDICAL CENTER

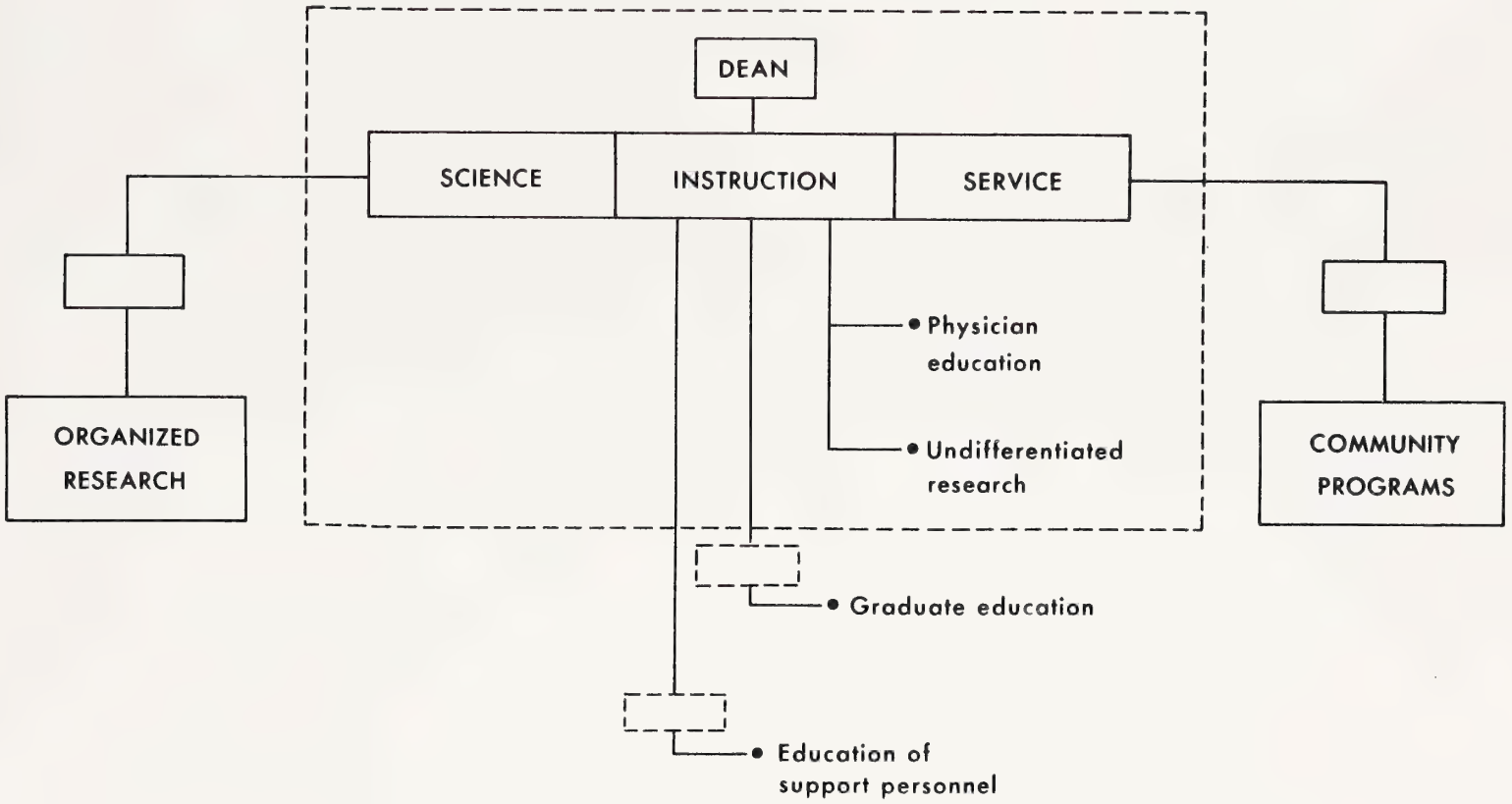
1967



*Broad Federal support

CHART 4

THE MODERN UNIVERSITY MEDICAL CENTER
Organizational Considerations



TAB 96

Photos: MWN/George Tames

The research philosophy of James A. Shannon, M.D.

James A. Shannon, who went to work for the National Institutes of Health 16 years ago as associate director for research and has spent the last 13 as director of the nation's principal biomedical research agency, will be 64 in August. He plans to retire on Sept. 1. During his tenure as director, NIH has grown from a less-than-$100-million/yr to a more-than-$1-billion/yr agency. Also during his tenure he has come under savage attack for alleged administrative shortcomings by the Fountain Subcommittee of the House of Representatives. Here he answers some questions put to him by William E. Small of SCIENTIFIC RESEARCH *about his philosophy of research at NIH.*

Dr. Shannon, one of the things we have been interested in is the apparent shift in the National Institutes of Health toward directed research—whether it's a shift, or an expansion of directed targeted research. How is this changing the make-up and the role of NIH?

Well, you would do science a great service if you clarify this because we have not discussed nor do we plan a shift from what can be called undifferentiated research to target research. Rather, we view it in a quite different context. At NIH, immediately after the post-war period, our research program was specifically targeted: work that had been initiated in the Office of Scientific Research & Development. The major problems that were undertaken had to do with very practical problems of doses and schedules of penicillin in the treatment of syphilis, and the very practical problem of completing the evolution of chemotherapeutic malaria agents initiated in World War II.

The period of 1956 through about 1962-64 represented a large growth in undifferentiated research, and the primary concern was to provide a very broad base determined by the scientific consensus about what is important. The medical sciences, being by their nature close to the applied area, contain a fairly broad mix of that which is fundamental and that which is applied.

At times the NIH has interfered with the natural evolution of this process, to provide a substantial broadening of the fundamental base for certain areas—as we've done, for instance, in physical biology and in genetics. At other times the effort has been directed toward application. Examples of this are cancer chemotherapy, virology of cancer, vaccine development, and the like.

There was also a change in emphasis in the mid-'50s. We decided that our training programs should emphasize almost exclusively the development of scientists rather than continue the development of medical specialists.

So, target research, or applied research, is not new to NIH.

Actually you're just reversing the phase, in a sense.

No, we're not reversing the phase at all. There has been throughout all this period deep concern for the application of new knowledge. And, if anything, there was, during the '50s and early '60s, an increase in the primary concern for the provision of a science base. If you go back over the hearings before the Congress during that period of rapid increase, the point was made repeatedly in the testimony and in both the House and Senate reports on the appropriation bills that if one were going to find answers to these very complex problems of disease—arthritis, atherosclerosis, hypertension, some of the chronic neurological diseases—one could not do that through the simple application of what was then contained within the knowledge base.

One had to find out what were the major science disciplines that had a capability of contributing to the knowledge base which at a later time would permit more applied studies. So the major thrust in that period was to reach into the basic sciences and bring them into the field of medicine: mathematics, physics, biophysics, biology and the like.

As this program developed, it became apparent in the late '50s and early '60s that there were certain areas where the knowledge base was adequate to permit exploration in a more organized fashion. Note, I'm saying here "exploring in a more organized fashion" rather than "undertaking targeted research."

What is the significance of that distinction?

Let me take one of the programs that's talked about so much in terms of targeted research: cancer chemotherapy, the first large program of organized research at NIH. It was clearly apparent by the late '50s that chemical substances

Shannon: We have not discussed nor do we plan a shift from what can be called undifferentiated research to target research.

are capable of providing remissions in acute leukemia of children. It was equally apparent that—except for early diagnosis, to be used with therapeutic skills, x-ray and surgery—the problem of cancer would not be resolved.

So organized approaches were developed to explore: 1. the relationship of various causative factors in the development of cancer (chemical first, viruses later), and 2. in a very broad fashion what the potential is for chemical substances to modify the cancerous growth. Basically, the working hypothesis there was to determine through such an exploration whether indeed the cure of cancer was feasible through the use of chemical substances. There was no thought that in a relatively short time these answers would be forthcoming. Indeed at that time there was no certainty one could achieve a cure by chemical substances.

But, first in choriocarcinoma of women, and later in the leukemias of childhood, it became quite obvious that some cancer is curable by chemical substances.

With the demonstration of this principle, and the discovery of some of the characteristics of the operation of chemicals within a cancer-containing system, certain general concepts began to emerge. The present cancer chemotherapy program is based on these principles.

You were ready at that time, then, to begin a program that would actually pin down a cure for cancer?

Hopefully, yes. But of course we realized that—since cancer was not a single entity, nor was it due to a single cause then fully appreciated by our people—we were entering a very complex area.

Extensive work with the leukemias, or the lymphomas generally, was warranted because this presented a good working model in the animal. Consequently, it was felt that progress could be made much more rapidly. At the same time, it was realized fully that the bulk of the cancers that kill are solid tumors, and we are now beginning to obtain an understanding of a peculiar problem presented by the solid tumor.

In the past year or two it has become apparent that as one subjects a solid tumor to a therapeutic agent—either because of the differing susceptibility of the cells in the center of the tumor as opposed to those on the periphery, or because of the differing nature of the cells in terms of age, rate of proliferation or blood supply—that the therapeutic attack on such a tumor can be viewed almost as an onion. Given one course of treatment, one peels off one layer from the outside; given another course, one peels another layer, and so on. We don't know if this model is going to work in precisely this way.

But there is evidence that tumors of a certain type do have therapeutic responses of this general sort. The problem now is to find agents that have unique capabilities for some of these solid tumors. Thus, whereas we may call all of this, in one sense, target research, what we are really trying to study is the very complex mystery of the life of the tumor—which really has an independent life of its own and its own independent biology, which must be understood if it is to be removed.

Then through purely undirected research you get to the point where you can recognize a number of approaches that might peel this layer from the onion?

No, I don't think it goes that way. Rather, there is a combination of undirected and directed research that is undertaken by complex groups in which there is a mix of surgeons and internists, pharmacologists and pathologists, virologists and chemotherapists. Such a group viewing the problem from different points of view selects the lines of investigation that appear most promising.

Such studies are costly. The simple therapeutic trials of specific substances when finally undertaken have to be under highly stylized conditions so that what a man does in Buffalo can be compared with what a man does in New York—or in Bethesda. This is organized research.

How is it decided at some given point actually to put extra dollars or extra attention on solving a problem and contracting for this work to be done?

Well, it's usually as the result of a series of observations. The decision to move more broadly into solid tumors was made at a time when it was felt that the study of the lymphomas, both from the standpoint of chemotherapy and of virology, was well established in the hands of very competent groups, and a body of information was available for application to the more complex problem of solid tumors. So this was a technical decision that was arrived at over a period of two or three years' consideration.

Sometimes, as I understand it, you also decide to go into one of these areas when no one is actually looking at it, but when research indicates that it might be a promising new area.

Yes. A very good example here is the long discussions that led to the establishment of programs for mental retardation by the Institute of Child Health & Human Development. (I'm talking about organized research, rather than target research—I think they're quite different.) This decision was made on the grounds that: 1. there were not adequate scientists in the field, and

Circle 31 on READER SERVICE CARD

2. the normal structure of the university and medical school was not apt to develop people who could provide the mix of skills necessary to approach the problems of mental retardation. So organized research in this case led to the establishment of about 12 research institutes, formally imbedded within the university or medical-school complex. These will bring to bear the many skills necessary to attack the problem in a comprehensive fashion.

In precisely the same way, we have recently organized a unit on environmental health sciences. There are many such hazards—air, water, radiation, and so on. And although PHS had many control programs in these areas, it is our conviction that the science base for developing an understanding of what the biological hazard is to the human was simply not present. This was the basis of the establishment of our Division of Environmental Health Sciences [SR, Feb '67, 38].

The problem is to identify our individual intoxicants or combinations of individual intoxicants, and determine their consequences to health. If we can at least define these, then we can form sound ideas about levels of safety. We have no basic knowledge for this at the present time.

Is this Environmental Health Division to become a focus for the government's look at the interface between the human and his environment? I notice they're going not only into pharmacology, toxicology, drugs, and so on, but smoking, air pollution. . . .

I'll put it this way: I think you become distracted if you use these as specific examples. The division's responsibility is to provide the knowledge base that permits an understanding of adverse effects that result from physical or chemical intoxication on biological systems. The peculiar nature of the problem stems from the multiplicity of agents involved; the generally low concentration of these agents; the tendency for this exposure to be of a long period.

Is biomedical engineering in the same category with child health and environmental health?

That's another one, yes.

What's happening there? We understand you are going to try to create a division or some central organization in the biomedical engineering field.

I'd rather not go into that now. I've also said that this would be a very costly area. In the first place, bioengineering is not a clearly-defined entity. Physical biology makes use of engineering techniques to explore biological systems —and this is very sophisticated, straightforward science. Then engineering skills are used for developmental purposes of a target nature, whether this be hardware or the establishment of complex informational systems or the examination of medical care systems. These are also generally considered to be in the engineering area.

We will continue to expand, as rapidly as we can, the use of engineering sciences in the exploration of biological mechanisms. We will limit our activities in engineering development to three major areas: 1. the myocardial insufficiency, artificial heart, or heart-assist devices program; 2. the system that contains chronic renal disease and hemodialysis; and 3. automation of laboratory procedures.

Does this third one include computers?

Well . . . it may or may not involve computers, depending upon whether the automation is computer-based, as is ours, or is free-standing with modified computational print-out.

We are well into these three areas of engineering development at the present time. The next critical stage is not only to support more developmental engineering, but to create some institutions that will provide the hybrids both to participate in programs of engineering development and to produce bright young engineers with a biological base.

The development work would be largely based in industry, and indeed so it should be. But since development problems in biology and medicine are much more complex than is the case in weaponry or space, it would be unwise for us to enter these fields aggressively unless we have the best talent available. This requires some certainty that there will be substantial growth. Until we are sure that such growth is possible, the program will develop slowly.

Along with the organized research, the targeted or contract research has also been increasing. Is there an attempt on the part of NIH to find solutions now?

Well, there always has been, but the opportunity is increasing and will increase in the future. As we consider these fields, we must be convinced of two things: 1. the knowledge base for the solution of the problem is present, and 2. at the same time, the problem is important.

But, finally, we fully realize that when one undertakes target research, areas of deficiency of knowledge not infrequently begin to turn up that may require the most fundamental type of research. Though it may be fundamental, it must have direct relevance to the solution of the problem.

After all, we are a categorically-oriented program. We're here because it's hoped that we can solve the problems of serious disease. It so happens we also have conviction that the knowledge base is inadequate to provide a platform for the development of much in the way of target research.

So our skill as administrators is determined in no small measure by maintaining adequate balance among that which is fundamental, that which is applied and that which is developmental. And since the last in general is very costly, you want to be sure the knowledge base is there, that you are really willing to take it and develop it in an intelligent manner.

Dr. Shannon, what are your plans for the future?

In the next few months, I will be primarily concerned with establishing priorities that will permit us to accommodate to both the 1968 and 1969 budgets, taking another look at our organization—including first-phase planning for the new Fogarty International Center, and with general administrative problems.

What are your personal plans?

I think that this gives me enough work to do for the next couple of months, and I'll start worrying about myself after these problems are settled.

You haven't any plans, then, after leaving NIH?

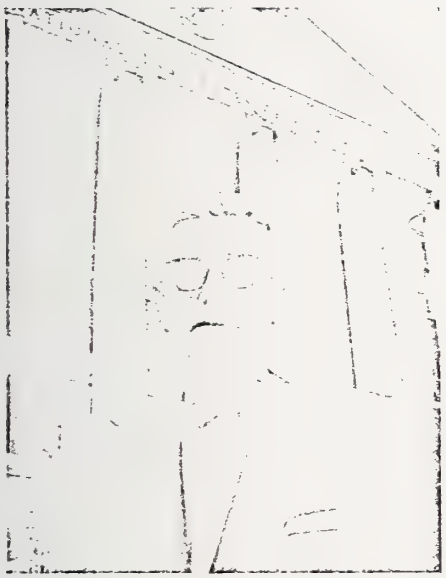
None whatsoever.

SCIENTIFIC RESEARCH • JANUARY 8, 1968

TAB 97

Federal Research

JAMES A. SHANNON, M.D.
Director, National Institutes of Health



The National Institutes of Health continued to mount an intensive and diversified effort in its own laboratories and to support a wide range of scientific endeavor through its extramural program.

One challenge last year was posed by recognition of the emerging opportunities in bioengineering. NIH scientists acquired a new awareness of the quantitative and analytic potential of engineering, and the NIH engineers profited, in turn, from their confrontation with the complexities of the life sciences. The results were clearly visible in several areas.

The automated clinical pathology laboratory of the Clinical Center was expanded to include ten fully automated on-line chemical tests in real-time mode and eight semiautomated on-line hematologic tests. Results of some 200 manual chemical tests are entered into the computer program by mark-sense punch cards, while the manual hematologic tests are entered via specially designed remote-entry keyboard consoles in various laboratories. Automated procedures for antibiotic sensitivity, serology, and urine cultures are under study.

In the Division of Computer Research and Technology, methods of data storage and retrieval were being developed and refined to make information available rapidly and selectively to NIH investigators. Other services provided by this division included development of statistical methods that make it possible to draw meaningful conclusions from great masses of data, advice as to the kind and volume of data that must be collected to yield statistically valid results, and guidance in analyzing data.

Furthermore, the National Institute of General Medical Sciences supported a variety of extramural bioengineering projects. Some of these aimed at using ultrasound as a diagnostic tool, at developing tiny implantable devices to record the electrical activity of the heart and other muscles during normal patient activity, and at devising microelectronic automatic control systems for various parts of the body. These systems could be of incalculable benefit to stroke patients, amputees, and those who have various other disabilities.

For the fiscal year July 1, 1967, to June 30, 1968, Congress appropriated $1,178,924,000 to the NIH. The appropriation was signed by the President on November 8 of last year, but as of this writing final allocations have not been made.

The following are the amounts appropriated to the individual institutes: National Cancer Institute, $183,356,000; National Heart Institute, $167,954,000; National Institute of General Medical Sciences, $160,284,000; National Institute of Arthritis and Metabolic Diseases, $143,954,000; National Institute of Neurological Diseases and Blindness, $128,633,000; National Institute of Child Health and Human Development, $68,621,000; National Institute of Dental Research, $30,307,000. The Division of Biologics Standards is to receive $8,649,000.

Fogarty Center Funded

The Congress earmarked $500,000 of the NIH appropriation for the initial planning of the John E. Fogarty International Center for Advanced Study in the Health Sciences. A small portion of this appropriation will be used for renovating an existing NIH building to house the center temporarily.

When completed, the Fogarty center will have space for conference and assembly rooms with facilities for simultaneous translation, living quarters for resident scholars, and overnight accommodations for participants at conferences. It will be the site of international conferences and seminars to explore major scientific problems and their social implications and to review developments that have occurred in research.

As envisioned by the late Representative Fogarty, the center also will provide facilities and support for a limited number of scholars, who will be in residence for advanced study for periods of one or two years. International fellowships will be offered to outstanding individuals for research, teaching, and study in the health-related sciences. The center will also serve as a central reception point for the large number of scientists, guest workers, and other foreign representatives who visit the NIH. **J.A.S.**

TAB 98

THE EXTENDED LIFE SPAN: HEALTH FACTORS*

James A. Shannon, M.D.,** and Stuart M. Sessoms, M.D.***

When Dr. Shannon found that he wouldn't be able to attend today, he asked me to express, first, his deepest regrets. He also asked me to convey his appreciation, which I share, for the opportunity to take part in this symposium on what we regard as a vital issue.

Although the title of this presentation is a generous one, inviting broad generalizations on the implications of our extended average lifespan, Dr. Shannon and I are aware that the Centennial Symposia, past and projected, all involve the problems of the city and the urban university. This, then, has focused our attention on the "vital issue" I have mentioned: the conditions that underlie the changing mortality picture, with particular reference to the problems of the central city, and the role of the university in modifying those conditions in the interest of better health for more people.

This may seem diffuse; but I believe it can be shown that the themes are interrelated in a specific way.

The lengthening of life during the present century is largely a reflection of declining infant mortality, particularly in certain advantaged population groups. In the central city, we see a persistence, or even a renewal of the more primitive conditions. Thus, analysis of the changing lifespan inevitably poses two sets of problems:

- First, the effects of high infant mortality. This is sharply contrasted with high infant survival, both within the greater urban setting--a situation with critical overtones in the delivery of health services.

- Second, a prevalance of older people with their peculiar health disorders, which may be complicated by the socio-economic characteristics of the city.

---

*Prepared for presentation at Wayne State University, Detroit, Michigan, February 15, 1968.

**Director, National Institutes of Health, Public Health Service, U.S. Department of Health, Education, and Welfare.

***Deputy Director, National Institutes of Health, Public Health Service, U.S. Department of Health, Education, and Welfare.

Attention to the university in this connection follows naturally, for therein lies the main hope for essential knowledge, guidance, personnel, and direct engagement with the problems--and those problems are among the gravest facing society today.

Let's take a look now at the demographic data and the forces that have influenced them since the turn of the century. The U.S. population is changing in two major ways--composition and growth in size. These changes represent the interaction of three factors--mortality, fertility, and population movements.

- The net effect on total population is that we are growing larger at the rate of about 2.8 million per year.

- The net effect of mortality changes on longevity are that the average lifespan has increased, as illustrated by the facts that the child born in 1920 had a life expectancy of 54 years; in 1964, 70 years.

- One implication of the change in average lifespan is a rising proportion of older people. For example, the percent of total population 65 years-old-and-over was 6.8 in 1940, 8.1 in 1950, 9.2 in 1960. It is now about 9.5 and still rising.

But we want to examine the total picture a little more closely to see what is chiefly responsible for the longer average lifespan. During the past 50 years, and especially the past 20, there has been little change in the overall death rate. Yet the U.S. population has doubled. This can be attributed to the rates of birth and infant survival. Progress against fatal diseases of children and youth is reflected in chart 1. Our analysis of the material is based on papers by Walsh McDermott, Professor of Public Health at Cornell University Medical College.* The chart shows the age-adjusted death rate between 1900 and 1960. To arrive at the age-adjusted death rate, corrections were made to maintain the age distribution that prevailed in the base year 1940. Thus the shaded area represents the deaths that would have obtained if the population each year had contained comparable numbers of young and old.

Except for the impact of influenza in 1918, the curve shows a steady decline during phase I (1900-1937), then a sharper drop through II (1937-1954), and then a leveling off. We can see how the age-adjusted rate starts a sharper decline in 1937, pulling away from the less-revealing crude rate indicated by the dotted line. This shift reflects the introduction of antibiotics--namely, sulfonamide, which was soon to be followed by a host of life-saving drugs. One can rightly call this a triumph of science and medicine, largely administered by the individual

---

*Environmental Factors Bearing on Medical Education in the Developing Countries, J.Med.Ed., 41,No. 9(2):137-174, September 1966.

physician. The leveling off in phase III, which has prevailed since 1954, is due to medicine's collision with the tougher nuts to crack--the chronic degenerative diseases of the cardiovascular system, malignant diseases, etc.

Chart 2 will provide an insight into the health status of the inner city. This shows infant mortality by prominent causes in New York City between 1900 and 1930 (rates per 1000 live births). Two facts should be noted:

- One, the decline in infant mortality during this period is largely due to a decrease in deaths from digestive and respiratory diseases--the "diarrhea-pneumonia" complex. These accounted for half of the infant deaths at the turn of the century.

- The second fact is that impressive decreases occurred in this disease complex prior to the availability of antibiotics. The period, however, is marked by a general improvement in living conditions and the introduction of public health measures--chlorination of drinking water, pasturization of milk, etc.

In other words, the great improvement in infant mortality during this period, and thus the beginning of a much-lenthened lifespan, was due to socio-economic advances and general public-health measures rather than the individual delivery of health services. And this occurred in the absence of specific drugs or vaccines for the major killers of that time. Since 1930, there has been a continuation of the decline in total infant mortality to its present 24 or 25--a vast improvement over the 140 deaths per 1000 live births shown at the left of the chart. The gradual shift from a "managerial physician system" to an "individual physician system," to use Dr. McDermott's terms, reflected further advancement in overall living conditions as well as in the physician's capability.

Now, to recapitulate briefly before moving on:

- A decline in the infectious diseases of children and young adults has brought an extension of the average lifespan. In comparing the leading causes of death in 1900 and today, one notes most emphatically that respiratory diseases--pneumonia, influenza, tuberculosis--and digestive diseases of the newborn have declined, while an aging population is now encountering heart disease, cancer, and stroke.

- Despite the lengthening of the average lifespan from birth, the life expectancy of the middle aged has been little improved. Chart 3, using data for 1900 and 1960, shows that the total lifespan has risen about 21 years since 1900, but that the life expectancy of 45-year-olds is higher by only 4 years.*

---

*By 1964 the average lifespan from birth and from age 45 were about the same (70.2 years and 29.7 years). The life expectancy of the average male of 45 was 27.1 years, and the average female 32.5 years.

- Finally, we have seen how a gradual decline in mortality, due to social as well as public health progress, was followed after 1937 by a sharper decline due to scientific advances, until a plateau was reached about 1954.

Our growing group of older people presents, from the medical standpoint, a twofold problem:

- First, disability, such as orthopedic disablement, blindness, and deafness, which are disproportionately prevalent in the older population. Medicine is charged with the task of keeping the older people healthy and productive.

- Second, chronic disease mortality. The aim is to add years of useful life through medical research and disease control. Here, it might be pointed out that the quality of life, not the quest of immortality, is the primary concern.

I have intimated that national or city-wide mortality figures are of limited value in studying the problems of special groups, such as the inhabitants of the inner city. And it is among the disadvantaged groups that health problems today are most acute. In the worst slum areas of our cities, the mortality of infants is tragically high. In New York City, for example--where the overall infant mortality compares with the national average of about 25--there are neighborhoods where the rate is as high as 47 and others where it approaches 12. The difference of 33 annual deaths per 1000 infants under 1 year of age must be considered excessive.

The implications of this distressing situation go beyond the personal tragedy of the infant's death. For every one that dies, there are many who suffer on with the same conditions, in some cases impaired for life. Recent studies indicate that children may be entering school with subtle brain damage from an illness of infancy.* Moreover, high infant mortality in a culture of poverty may actually contribute to attitudes that favor high birth rates, through biological overcompensation, the cheapening of life, and general hopelessness. The transfer of rural culture to an urban setting, which to some extent characterizes the inner city, may also promote births. Obversely, to effect the reduction of births on a broad scale requires conditions in which high fertility seems a disadvantage rather than an advantage to individual couples.

It is also clear that the high birth rate in a culture of poverty contributes to overcrowding, poor hygiene, dietary deficiency, and other conditions that lead to high infant mortality. Thus a vicious circle is established that is very difficult to break.

---

*Luttrell and Finberg, 1959; Birch, H., 1966; Eichenwald, H., 1966.

The problem I have sketched in its barest outline--the vicious circle of high-fertility/high-mortality/high-fertility--poses special problems for medicine. Possible approaches include birth control, medical and public health measures, and dietary improvement. Education and economic aid are necessary prerequisites at all these points. And we must note particularly that the core of the infant-mortality problem, the "diarrhea-pneumonia complex," is not highly amenable to medical control through the individual-physician approach. It will be recalled that these diseases, so prevalent nationally before 1930 and common today in both the inner city and the underdeveloped nations, were declining in the United States before medical services of the type featured in today's society were available in a significant way. But the sociologic and hygienic conditions that were mainly responsible for the improvement cannot readily be delivered to the ghetto.

So one could group the main health problems of America today into two broad categories--problems of the inner city and the abiding problem of chronic disease. Let me address myself more specifically to the needs these impose.

The health problems of the inner city--particularly the high infant mortality, but also to some extent the drug abuse, alcoholism, venereal disease, tuberculosis, and disease and disabilities associated with aging--must be approached on the basis of socio-economic changes combined with the delivery of health services. A substantial array of preventive and therapeutic measures are available. But these will be inadequate without broad improvement in social and economic circumstances, better education, better hygiene, family planning, prevention of early pregnancy--all the changes that have occurred in the advantaged areas.

With regard to chronic disease, we cannot talk meaningfully of eradication, in any case, as with the infections and nutritional deficiencies. We can only look realistically toward deferral, at least in the foreseeable future. We are just beginning to understand atherosclerosis, hypertension, cancer, diabetes, and are doing as well as possible with the means at hand. We need to know a great deal more about the nature of diseases. To reach and help the increasing numbers of people, we need new tools, new medicines, and new methods.

Now, where does the university fit into all this? One role concerns the delivery of health services. The ghetto has an economy that precludes development of private medical practice in the usual sense. Hence, something new appears to be necessary.

In other words, it is most unlikely that the private practitioner will move by choice into areas of high population density, low income, and lack of local resources. But some of the medical schools can serve as a base for broad extension of high-quality services to ghetto areas in a direct and purposeful way. The program of Albert Einstein College of

Medicine, with its strategic relationship to the Bronx, is a case in point. One may hope that the medical schools, professionally and scientifically, can provide the base for development of a new attack. The medical centers are charged, of course, with increasing the essential personnel--physicians and supportive workers. These will be needed, even if the new attack involves a high component of managerial approaches to the delivery of services rather than the more traditional individual approaches.

Quite apart from sociology (except in regard to deployment of personnel and other resources), the universities and their medical centers face the need--

- To provide adequate numbers of qualified physicians, nurses, and other auxilliary workers.

- To reestablish and maintain stability--namely, a proper balance among education, services, and research. The continued independence of the medical school will depend on financial stability, which can only derive, as Dr. Shannon and I see it, from major Federal assistance.

- Lastly, the need to pursue not only medical research of the traditional types, but also studies and experiments in the delivery of services, ranging from hospital systems to the development of new ancillary cadres to better methods for the continuing education of physicians.

In other words, the university must engage with the problems of the city in bold new ways.

One final point: society, and particularly government, must come to regard the university as a national resource. In Dr. Shannon's words: "Unless the university comes to be viewed as a precious national resource, and given the necessary support for education, research, and social responsibility, we aren't going to make it."

Chart 1.

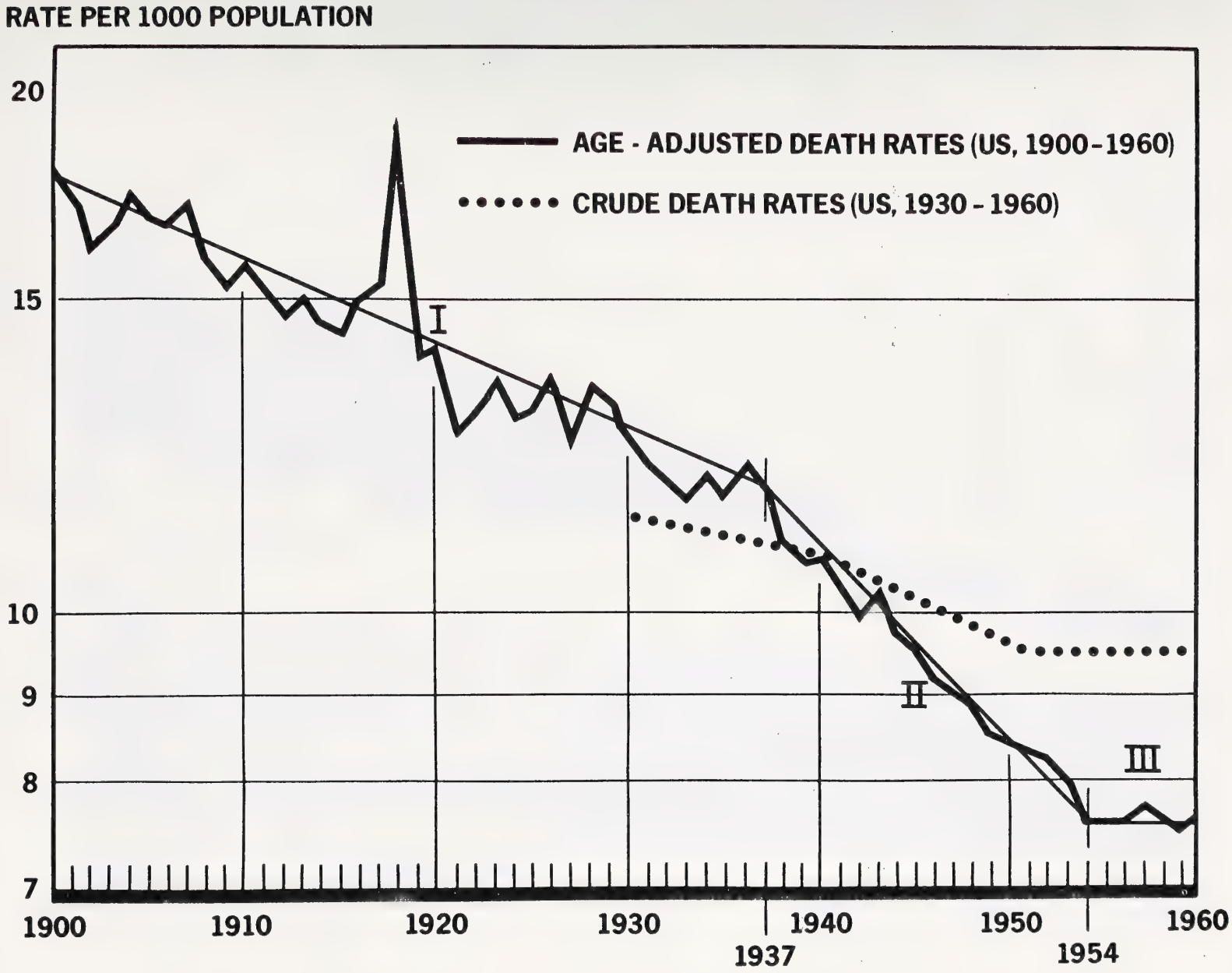

RATE PER 1000 POPULATION
20
15
10
9
8
7
AGE - ADJUSTED DEATH RATES (US, 1900-1960)
CRUDE DEATH RATES (US, 1930 - 1960)
I
II
III
1900
1910
1920
1930
1937
1940
1950
1954
1960

Chart 2.

INFANT MORTALITY BY PROMINENT CAUSES
RATES PER 1000 BIRTHS
NEW YORK CITY

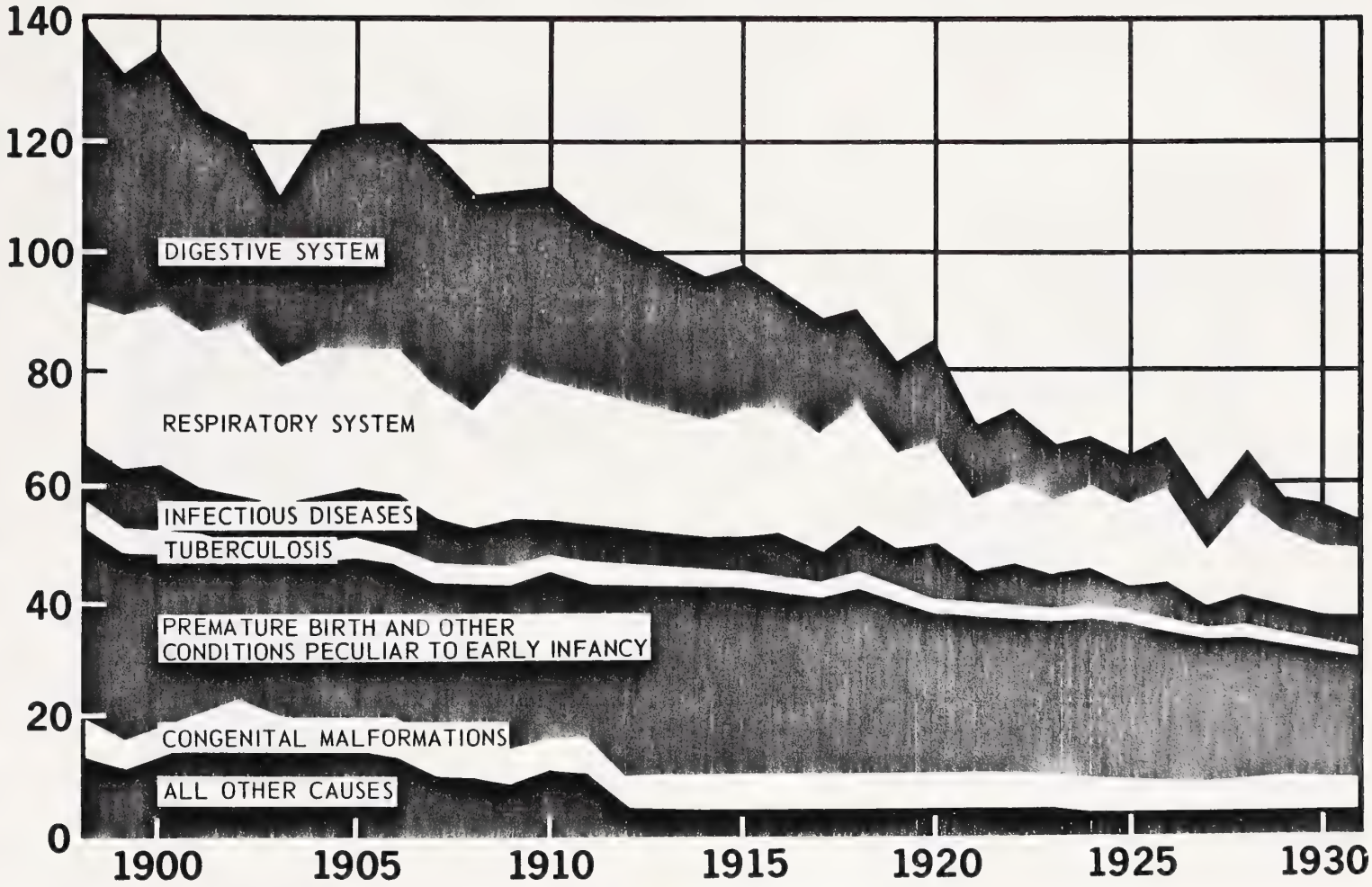
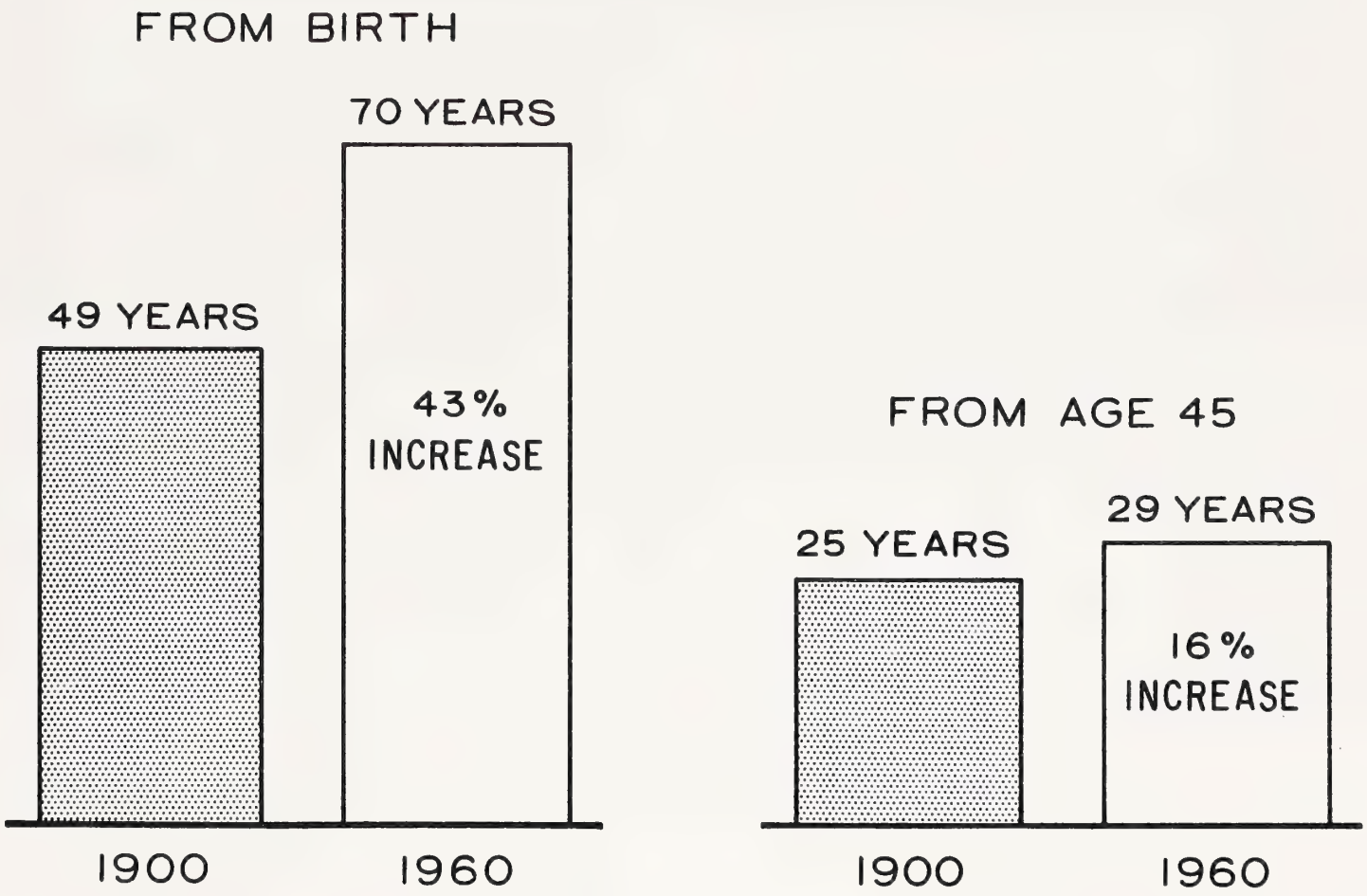


Chart 3.

LIFE EXPECTANCY



TAB 99

March 14, 1968

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE

Statement by Director, National Institutes of Health
Public Health Service

on

1969 Appropriation Estimates

Mr. Chairman and Members of the Committee:

The aims and accomplishments of the specific activities supported by the thirteen separate appropriations for the National Institutes of Health will be discussed by the Director of the Institute or Division directly responsible for the activity. My comments are therefore devoted to general problems and will reflect the current deployment of our resources rather than what might be needed for the full exploitation of the opportunities before us.

The budget estimates for the Fiscal Year 1969 are Spartan in character. Over-riding circumstances, resulting from national commitments elsewhere, have necessarily limited the President's estimates for NIH to the amounts that can be justified as essential if the medical research establishment of the nation is to be maintained at an effective level and not be threatened with disarray and deterioration. The 1969 budget is based on 1968 obligations that are some $50 million lower than the amount appropriated by the Congress. The budget thus continues to be

responsive to the Congressional directive that a conscientious effort be made to reduce expenditures. In fact, I believe that it is as lean a budget request as is possible without seriously curtailing the activities which it is designed to support.

The budget does provide for the orderly continuation of the major existing programs and will enable us to meet our existing commitments to the research community. With some rearrangement of plans, it will also allow a minimal expansion of a few of the most critical activities. Frankly, it is not a budget that makes me happy but it is a budget that maintains a critical mass of biomedical activity while we plan for the most effective deployment of resources when dollar constraints on these activities will be less severe.

Last year--because several members were new to this committee--I contrasted the tremendous advances that have been made in medicine with the tremendous gaps that still exist in our knowledge of biological processes and in our ability to deal with disease. And I outlined the development of the NIH programs which are now contributing so significantly to narrowing these gaps. This year--because it is the last time that I shall appear before this committee to defend the NIH budget--I should like to talk briefly about the future directions of these programs and to point out some problems that require special attention within the framework of present budget limitations.

Population

One of the areas of research activity that has particular national significance is population control and fertility studies. Although exact figures are not available, it was estimated in 1950 that the world's population was 2.5 billion. By mid-1966, the estimate had risen to 3.4 billion--a 36% increase in 15 years. At this rate there will be between 7 and 8 billion people by the year 2000--which is only 32 years away. Many very serious economic, social, and geopolitical problems are raised by these figures--and by the probable geographical distribution of these staggering increases. These problems are not problems that fall within the purview of NIH and I do not propose to discuss them. Nor can policy decisions to deal with these problems be medical decisions; they must be social and political decisions. However, medical research will continue to contribute to this population explosion both by saving more babies and by keeping more older people alive. It also has a responsibility for developing an adequate science base for meaningful population control. Despite recent advances, readily acceptable and generally applicable antifertility techniques are not available.

Population control and fertility studies should not be solely concerned with their negative aspects. There is as great a need for solving the puzzling problems of infertility as the relatively much simpler problem of rampant fertility. The whole process of

human reproduction is, in fact, very imperfectly understood. The successful unravelling of its mysteries would not merely solve problems of infertility and provide for both family planning and population control but would undoubtedly enable us to prevent many of the birth defects that now take such a tragic toll. Nevertheless, the problems created by an uncontrolled population expansion are of such severity that our major initial emphasis must be on this aspect of research on human reproduction.

Behavioral Sciences

The whole field of the behavioral sciences also needs intensive exploration from a biomedical point of view. Questions concerning growth and development, the behavioral determinants of biological systems, individual behavioral pathology and social pathology, all fall within the joint domain of the social and medical sciences. The definition of health and the consequent domain of responsibilities of medical research have been considerably broadened over the past decades. They now include, in addition to the biological factors which contribute to health, the need to understand the emotional, intellectual, economic and social factors which exert such a dominant influence on well-being. Yet the scientific basis for understanding many of these factors may be described as being in the same relatively primitive state as was biological knowledge in the 1930's. There is an obvious need to incorporate the behavioral sciences--and,

indeed, the broad spectrum of the social sciences--into the medical establishment. Such a move will have a broadening influence not only on research capability but on the educational experience to which developing physicians are exposed.

Heretofore, the major contribution to the support of the behavioral sciences has been limited to the National Institute of Mental Health. The establishment of NIMH as a separate bureau in the Public Health Service will not diminish the need for it to maintain its present role in the support of the behavioral sciences. However, the behavioral sciences also have increasing importance and relevance to the programs of other Institutes.

The National Institute of Child Health and Human Development provides avenues for developing many aspects of the behavioral field related to normal human growth and development. It has now become possible to support research and training programs in the emotional and intellectual development of the normal child in parallel with studies on biological development. An understanding of the learning process, cognitive processes, development of speech, and linguistics are significant areas of investigation. Similarly, the research and training of scientists in the field of aging--and the elucidation of the attendant intellectual, emotional and social problems --are essential.

The National Institute of General Medical Sciences is responsible for providing a wide-ranging discipline-oriented base for research and training in the behavioral sciences in order to underpin the more limited problem areas of each of the categorical Institutes. This Institute will stimulate the growth of behavioral-science research and training in the medical schools and facilitate its inclusion in the core curriculum of medical education.

The Regional Medical Programs also serve to focus attention on problems which have heretofore received insufficient attention--the sociology and economics of medical care, including questions of new medical care systems, problems of acceptance of medical information and services, needs of the physician as well as the patient, and the effects which affluence and education are having on the demand for medical services.

Bio-engineering

The third area that merits more purposeful development is the broad field of bioengineering which may be briefly defined as the application of the principles and techniques of the physical --and especially the engineering--sciences to medicine and to the problems in biomedical research leading to more effective diagnosis and treatment of disease. Dramatic examples of what has already been accomplished in this field are the development of an array of monitoring devices, automated laboratory devices, the so-called artificial kidney, the development of the heart-lung machine,

the pace-maker, and artificial heart valves, and the work now in progress on the artificial heart. There are also many less publicized but equally important contributions of bioengineering to clinical care and to both clinical and laboratory research. However, the potentialities of this field have hardly begun to be exploited. A systematic and vigorous pursuit of the application of engineering know-how--especially the recent advances in electronics, miniaturization, and the creation of new materials --can, I feel sure, bring about startling changes in diagnostic and therapeutic methods and in the organization of hospitals and hospital systems during the next 10 or 20 years.

In the FY 1967 appropriations, this committee provided $250,000 for "planning, consultation and contractual assistance" with a view to launching bioengineering development as "a distinct activity for which separate budgetary provision should be made". With these funds we awarded a contract to the National Academy of Engineering for a study which will examine the future role of engineering concepts and technology in

... advancing the understanding of biological systems,

... the development of instrumentation, diagnostic and therapeutic devices, artificial organs, etc., and

... the evolution of hospitals and health service units.

Recommendations will also be made for converting the no-man's land that lies between engineering and biomedicine into a recognized hybrid activity and for developing prototype

institutions that can foster research and manpower development in this new field.

A first report on the results of this study is expected within the next month. It is, however, already clear that a beginning on a systematic and integrated program must be made now and the budget request therefore includes $1.3 million for this purpose. To defer the beginning of this activity--which is already long overdue--would be most unwise.

Research Institutes

Another aspect of the NIH programs that needs selective development is the creation of new institutional foci for research on broad problems that are particularly promising but which require a combination of scientific talent and facilities not commonly found even in the best of our medical schools and associated hospitals. The need for special research-institutes for this type of activity is clearly defined in the report of the President's Commission on Heart Disease, Cancer and Stroke. But they are not only needed for the three disease areas covered by that report but also for the dental sciences, for aging, for pharmacology and toxicology, and for bioengineering --to mention only the more important areas. Such centers, which will provide a suitable setting for complex research activities and special science development, are essential if we are to move progressively to a more direct approach to the solution of service problems, now that a broad science base for a number of developmental

activities has been established. A recent re-examination of our plans for expanding federally-initiated programs for collaborative research on specific health problems--to which I shall come back in a moment--has clearly revealed that many of these projects will be greatly assisted by more purposeful programming but that it is unlikely that this can be done in the conventional university or medical school department without either distorting the project or distorting the total research and educational program of the institution. This is particularly true, for example, of the development of the artificial heart and, probably, in general of bioengineering projects; of the cardiovascular and stroke centers; of the proposed fertility studies; and possibly of some of the behavioral studies that need to be undertaken. Provision will have to be made in future for establishing research centers specially equipped, staffed and located for the most effective pursuit of some of these sharply defined and carefully directed studies. They will also provide a new dimension in the training of a new type of scientist for the future. However, it must be emphasized that such activities cannot replace the broad base of scientific effort already established because this provides the fertile soil on which developmental opportunities grow.

Institutional Problems

A more general problem--which is becoming urgent as a result of the mounting economic pressures on our educational institutions

--is the need to make more realistic provision for maintaining the stability of the institutions in which the bulk of our medical research and research training must continue to be conducted. Federal support for these activities now plays such a major role in institutional financing, staff deployment, and program planning that even relatively minor shifts in Federal programs or restrictions in the availability of funds can have a major impact on these institutions. For example, we have found that the normal operation of our priority-scoring system, under the present availability of funds for competing research projects, threatens in some cases to wipe out all NIH support for a medical school department which has been heavily dependent on that support. This, in turn, has an indirect but profound effect on the educational system. Clearly, a more stable system of support--or, at least, one less subject to rapid fluctuation --must be established if some of our medical schools are not to be seriously harmed.

Indeed, the institutional problems of medical education should be a matter of urgent national concern and merit the most serious consideration. The responsibility of the medical schools and their faculties has broadened considerably since the early 1950's. Physician education has become more deeply rooted in fundamental science; there has been a rapidly expanding need to provide post-doctoral and specialty training; research has become more complex, more technologically demanding, more essential to

the educational process, and much more expensive. These changes have created a need for more diverse skills on the part of the faculty; have to some extent out-moded the traditional curriculum for physician education; have placed great strains on the organization of the medical schools; and have weakened the bonds between the faculty, the institution and the students.

These strains and stresses are now being compounded by the progressive involvement of the medical schools in organized research projects aimed at the achievement of nationally desirable goals and by the greater demands society is now making for the right of every individual to adequate health services irrespective of his economic status or the remoteness of his place of residence from centers of medical excellence. The growth of combined research and service programs in rehabilitation, mental health and mental retardation; expanded service programs for children; the development of comprehensive community health programs for economically disadvantaged groups; participation in the Regional Medical Programs and in the Partnership for Health; involvement in operational research on the development of more effective delivery systems for health care; and the obligations imposed by Medicare and Medicaid are now over-burdening academic institutions that were initially designed for the relatively simple task of providing a well-ordered and traditional education but which are now being called upon to deal with a broad array of social problems.

We have given serious consideration to these institutional problems which vitally affect the NIH programs and we are in process of developing some proposals for their solution in association with the Bureau of Health Manpower.

Review of Special Programs

I have already referred to an intensive review which we have conducted during the past month or so of the Federally-initiated research programs sponsored by NIH. There are about 30 of these so-called 'organized' research programs. Our objective has been to review their present status, assess their potential growth, and to develop plans for their future development. These plans are not 'blue-sky' proposals but conservative projections based, first of all, on what it appears feasible actually to achieve and, secondly, on the 5-year targets that were approved by the Department last spring. If circumstances permit--that is, if financial constraints are relaxed--these reviews will provide a realistic basis for orderly program development in these areas during the next 4 or 5 years.

I should like to emphasize that these Federally-initiated programs are not being undertaken at the expense of the investigator-initiated research projects which have dominated NIH support up to the present time. They do not represent a change in NIH policy but an addition to NIH activities which should serve to provide a more effective approach to selected problems of major national importance that justify--or, indeed,

require--more organized collaboration and programmed activity than is characteristic of research conducted by individual investigators.

These Federally-initiated programs will not displace support for investigator-initiated research which must continue to be the main stream of biomedical research. The traditional research projects represent the consensus of the scientific community on what is important and what is feasible. This self-generated activity--with its mix of fundamental and applied research--will not only ensure the vigorous pursuit of the widest range of opportunities for progress but must continue to provide the scientific under-pinning for developmental projects and to point the direction for future targeted research.

The increased attention being given to these 'organized' programs should also not be interpreted as representing any wavering of our firm conviction about the over-riding importance of so-called basic research. In fact, the 'organized' programs of today have been made possible by the basic research of yesterday. Their present feasibility and probability of success is directly proportional to the soundness and completeness of the fundamental research on which they are based. It follows, of course, that the feasibility of future 'organized' or developmental programs will depend almost entirely on the scope and vigor of the fundamental research that we support in the intervening years. It is certainly true in scientific research that you can't harvest what you don't first sow.

Heart Transplant Problems

The difficulties that can be encountered as the result of gaps in fundamental research are now dramatically illustrated by the recent spate of heart-transplant operations. While the purely surgical techniques for such operations are already well-developed, too little research has, as yet, been completed on tissue antigens, species differences in rejection phenomena, and organ differences in rejection in the same species. A great deal more knowledge is necessary before any of these operations can become an accepted form of treatment. In fact, the newly established Board on Medicine of the National Academy of Sciences issued a formal statement on February 28 describing these operations as "a scientific exploration of the unknown" and urging that "only a relatively small number of careful investigations involving cardiac transplantation...be done at this time". At the same time, the American College of Cardiology called a conference--tentatively scheduled to start on March 23 at Bethesda--"to grapple with the problems arising from the development of techniques for heart transplantation". We are at present reviewing NIH activities on problems related to heart transplantation and considering how the necessary research can be accelerated, including relevant training and the provision of essential services. NIH should obviously take this task seriously and it will probably be necessary to redeploy some of our available funds to make a more concerted attack on these

problems. We are also giving urgent attention to the moral and ethical issues raised not only by heart transplantation but by a number of clinical investigations involving human subjects.

Fogarty Center

I am happy to report that we are making good progress in developing plans for the John E. Fogarty International Center for Advanced Study in the Health Sciences for which this committee provided funds last year. We expect that a contract for architectual drawings will be awarded well before the end of this year. Meanwhile, we are making arrangements to renovate and remodel 'Stone House' which is the large and attractive residence of the former owners of part of the NIH campus. It sits on the site selected for the Fogarty Center and it seems to us to be both practical and prudent to incorporate it into the over-all design of the Center. As renovation can probably be completed in about three or four months, it will be possible to bring some of the Fogarty Center activities into being much sooner than if everything had to wait for the completion of the new building. We expect that the conference program and the scholars program can both be started early next year.

Budget Summary

The $1,196,693,000 requested for FY 1969 is distributed as follows among the five major activities of NIH:

... The $275.6 million requested for direct operations include $87.5 million for the conduct of research by the NIH scientific staff and $126.5 million for collaborative projects. The remaining $61.6 million is for other

direct operations, program direction, administrative services and management of the extramural program.

... Of the $652 million requested for research grants, nearly $165 million is for the special research grant programs--including $53 million for General Research Support--and $342 million is for non-competing research projects which we already have a commitment to support. This leaves $145 million for competing grant applications of which it is estimated that close to $91 million will be needed for supplemental awards for existing grants and for the renewal of projects to which continued support has not been committed. About $54 million will thus be left for the support of new grant projects --this is $6 million less than the amount appropriated for this purpose for FY 1968.

... The requests for training grants and fellowships total $197.8 million--an increase of $4.8 million over the FY 1968 appropriation.

... For Health Research Facilities Construction Grants, $8.4 million is requested. In addition, $12.2 million will be carried forward from the FY 1968 appropriation in accordance with the Bureau of the Budget's directive for curtailing FY 1968 expenditures.

... The request for the Regional Medical Programs is $62.9 million. This activity will also have a carry-over from FY 1968 of $30.9 million.

I believe that your review of these requests will confirm my earlier statement that this is as lean a budget as it is possible for us to present to you without a major cut-back in programs whose only purpose is to benefit the people of this country by strengthening the ability of the medical profession to diagnose, treat and prevent disease and disability.

TAB 100

April 1, 1968

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE

Statement by Director, National Institutes of Health
Public Health Service

on

1969 Appropriation Estimates

Mr. Chairman and Members of the Committee:

The aims and accomplishments of the specific activities supported by the thirteen separate appropriations for the National Institutes of Health will be discussed by the Director of the Institute or Division directly responsible for the activity. My comments are therefore devoted to general problems and will reflect the current deployment of our resources rather than what might be needed for the full exploitation of the opportunities before us.

The budget estimates for the Fiscal Year 1969 are Spartan in character. Over-riding circumstances, resulting from national commitments elsewhere, have necessarily limited the President's estimates for NIH to the amounts that can be justified as essential if the medical research establishment of the nation is to be maintained at an effective level and not be threatened with disarray and deterioration. The 1969 budget is based on 1968 obligations that are some $50 million lower than the amount appropriated by the Congress. The budget thus continues to be

responsive to the Congressional directive that a conscientious effort be made to reduce expenditures. In fact, I believe that it is as lean a budget request as is possible without seriously curtailing the activities which it is designed to support.

The budget does provide for the orderly continuation of the major existing programs and will enable us to meet out existing commitments to the research community. With some rearrangement of plans, it will also allow a minimal expansion of a few of the most critical activities. Frankly, it is not a budget that makes me happy but it is a budget that maintains a critical mass of biomedical activity while we plan for the most effective deployment of resources when dollar constraints on these activities will be less severe.

I have, on past occasions, contrasted the tremendous advances that have been made in medicine with the tremendous gaps that still exist in our knowledge of biological processes and in our ability to deal with disease. You are well aware--and in large measure responsible for--the development of the NIH programs which are now contributing so significantly to narrowing these gaps. This year is the last time that I shall appear before this committee to defend the NIH budget. And I must add, Senator Hill, that all of us at NIH were saddened by your decision that this is also the last time that you will be conducting these hearings. I should, therefore, like to talk briefly about the future directions of these programs and to point out some problems that require special attention within the framework of present budget limitations.

Population

One of the areas of research activity that has particular national significance is population control and fertility studies. Although exact figures are not available, it was estimated in 1950 that the world's population was 2.5 billion. By mid-1966, the estimate had risen to 3.4 billion--a 36% increase in 15 years. At this rate there will be between 7 and 8 billion people by the year 2000--which is only 32 years away. Many very serious economic, social, and geopolitical problems are raised by these figures--and by the probable geographical distribution of these staggering increases. These problems are not problems that fall within the purview of NIH and I do not propose to discuss them. Nor can policy decisions to deal with these problems be medical decisions; they must be social and political decisions. However, medical research will continue to contribute to this population explosion both by saving more babies and by keeping more older people alive. It also has a responsibility for developing an adequate science base for meaningful population control. Despite recent advances, readily acceptable and generally applicable antifertility techniques are not available.

Population control and fertility studies should not be solely concerned with their negative aspects. There is as great a need for solving the puzzling problems of infertility as the relatively much simpler problem of rampant fertility. The whole process of

human reproduction is, in fact, very imperfectly understood. The successful unravelling of its mysteries would not merely solve problems of infertility and provide for both family planning and population control but would undoubtedly enable us to prevent many of the birth defects that now take such a tragic toll. Nevertheless, the problems created by an uncontrolled population expansion are of such severity that our major initial emphasis must be on this aspect of research on human reproduction.

Behavioral Sciences

The whole field of the behavioral sciences also needs intensive exploration from a biomedical point of view. Questions concerning growth and development, the behavioral determinants of biological systems, individual behavioral pathology and social pathology, all fall within the joint domain of the social and medical sciences. The definition of health and the consequent domain of responsibilities of medical research have been considerably broadened over the past decades. They now include, in addition to the biological factors which contribute to health, the need to understand the emotional, intellectual, economic and social factors which exert such a dominant influence on well-being. Yet the scientific basis for understanding many of these factors may be described as being in the same relatively primitive state as was biological knowledge in the 1930's. There is an obvious need to incorporate the behavioral sciences--and,

indeed, the broad spectrum of the social sciences--into the medical establishment. Such a move will have a broadening influence not only on research capability but on the educational experience to which developing physicians are exposed.

Heretofore, the major contribution to the support of the behavioral sciences has been limited to the National Institute of Mental Health. The establishment of NIMH as a separate bureau in the Public Health Service will not diminish the need for it to maintain its present role in the support of the behavioral sciences. However, the behavioral sciences also have increasing importance and relevance to the programs of other Institutes.

The National Institute of Child Health and Human Development provides avenues for developing many aspects of the behavioral field related to normal human growth and development. It has now become possible to support research and training programs in the emotional and intellectual development of the normal child in parallel with studies on biological development. An understanding of the learning process, cognitive processes, development of speech, and linguistics are significant areas of investigation. Similarly, the research and training of scientists in the field of aging--and the elucidation of the attendant intellectual, emotional and social problems --are essential.

The National Institute of General Medical Sciences is responsible for providing a wide-ranging discipline-oriented base for research and training in the behavioral sciences in order to underpin the more limited problem areas of each of the categorical Institutes. This Institute will stimulate the growth of behavioral-science research and training in the medical schools and facilitate its inclusion in the core curriculum of medical education.

The Regional Medical Programs also serve to focus attention on problems which have heretofore received insufficient attention--the sociology and economics of medical care, including questions of new medical care systems, problems of acceptance of medical information and services, needs of the physician as well as the patient, and the effects which affluence and education are having on the demand for medical services.

Bio-engineering

The third area that merits more purposeful development is the broad field of bioengineering which may be briefly defined as the application of the principles and techniques of the physical --and especially the engineering--sciences to medicine and to the problems in biomedical research leading to more effective diagnosis and treatment of disease. Dramatic examples of what has already been accomplished in this field are the development of an array of monitoring devices, automated laboratory devices, the so-called artificial kidney, the development of the heart-lung machine,

the pace-maker, and artificial heart valves, and the work now in progress on the artificial heart. There are also many less publicized but equally important contributions of bioengineering to clinical care and to both clinical and laboratory research. However, the potentialities of this field have hardly begun to be exploited. A systematic and vigorous pursuit of the application of engineering know-how--especially the recent advances in electronics, miniaturization, and the creation of new materials --can, I feel sure, bring about startling changes in diagnostic and therapeutic methods and in the organization of hospitals and hospital systems during the next 10 or 20 years.

In the FY 1967 appropriations, this committee provided $250,000 for "planning, consultation and contractual assistance" with a view to launching bioengineering development as "a distinct activity for which separate budgetary provision should be made". With these funds we awarded a contract to the National Academy of Engineering for a study which will examine the future role of engineering concepts and technology in

... advancing the understanding of biological systems,

... the development of instrumentation, diagnostic and therapeutic devices, artificial organs, etc., and

... the evolution of hospitals and health service units.

Recommendations will also be made for converting the no-man's land that lies between engineering and biomedicine into a recognized hybrid activity and for developing prototype

institutions that can foster research and manpower development in this new field.

A first report on the results of this study is expected within the next month. It is, however, already clear that a beginning on a systematic and integrated program must be made now and the budget request therefore includes $1.3 million for this purpose. To defer the beginning of this activity--which is already long overdue--would be most unwise.

Research Institutes

Another aspect of the NIH programs that needs selective development is the creation of new institutional foci for research on broad problems that are particularly promising but which require a combination of scientific talent and facilities not commonly found even in the best of our medical schools and associated hospitals. The need for special research-institutes for this type of activity is clearly defined in the report of the President's Commission on Heart Disease, Cancer and Stroke. But they are not only needed for the three disease areas covered by that report but also for the dental sciences, for aging, for pharmacology and toxicology, and for bioengineering --to mention only the more important areas. Such centers, which will provide a suitable setting for complex research activities and special science development, are essential if we are to move progressively to a more direct approach to the solution of service problems, now that a broad science base for a number of developmental

activities has been established. A recent re-examination of our plans for expanding federally-initiated programs for collaborative research on specific health problems--to which I shall come back in a moment--has clearly revealed that many of these projects will be greatly assisted by more purposeful programming but that it is unlikely that this can be done in the conventional university or medical school department without either distorting the project or distorting the total research and educational program of the institution. This is particularly true, for example, of the development of the artificial heart and, probably, in general of bioengineering projects; of the cardiovascular and stroke centers; of the proposed fertility studies; and possibly of some of the behavioral studies that need to be undertaken. Provision will have to be made in future for establishing research centers specially equipped, staffed and located for the most effective pursuit of some of these sharply defined and carefully directed studies. They will also provide a new dimension in the training of a new type of scientist for the future. However, it must be emphasized that such activities cannot replace the broad base of scientific effort already established because this provides the fertile soil on which developmental opportunities grow.

Institutional Problems

A more general problem--which is becoming urgent as a result of the mounting economic pressures on our educational institutions

--is the need to make more realistic provision for maintaining the stability of the institutions in which the bulk of our medical research and research training must continue to be conducted. Federal support for these activities now plays such a major role in institutional financing, staff deployment, and program planning that even relatively minor shifts in Federal programs or restrictions in the availability of funds can have a major impact on these institutions. For example, we have found that the normal operation of our priority-scoring system, under the present availability of funds for competing research projects, threatens in some cases to wipe out all NIH support for a medical school department which has been heavily dependent on that support. This, in turn, has an indirect but profound effect on the educational system. Clearly, a more stable system of support--or, at least, one less subject to rapid fluctuation --must be established if some of our medical schools are not to be seriously harmed.

Indeed, the institutional problems of medical education should be a matter of urgent national concern and merit the most serious consideration. The responsibility of the medical schools and their faculties has broadened considerably since the early 1950's. Physician education has become more deeply rooted in fundamental science; there has been a rapidly expanding need to provide post-doctoral and specialty training; research has become more complex, more technologically demanding, more essential to

the educational process, and much more expensive. These changes have created a need for more diverse skills on the part of the faculty; have to some extent out-moded the traditional curriculum for physician education; have placed great strains on the organization of the medical schools; and have weakened the bonds between the faculty, the institution and the students.

These strains and stresses are now being compounded by the progressive involvement of the medical schools in organized research projects aimed at the achievement of nationally desirable goals and by the greater demands society is now making for the right of every individual to adequate health services irrespective of his economic status or the remoteness of his place of residence from centers of medical excellence. The growth of combined research and service programs in rehabilitation, mental health and mental retardation; expanded service programs for children; the development of comprehensive community health programs for economically disadvantaged groups; participation in the Regional Medical Programs and in the Partnership for Health; involvement in operational research on the development of more effective delivery systems for health care; and the obligations imposed by Medicare and Medicaid are now over-burdening academic institutions that were initially designed for the relatively simple task of providing a well-ordered and traditional education but which are now being called upon to deal with a broad array of social problems.

We have given serious consideration to these institutional problems which vitally affect the NIH programs and we are in process of developing some proposals for their solution in association with the Bureau of Health Manpower.

Review of Special Programs

I have already referred to an intensive review which we have conducted during the past month or so of the Federally-initiated research programs sponsored by NIH. There are about 30 of these so-called 'organized' research programs. Our objective has been to review their present status, assess their potential growth, and to develop plans for their future development. These plans are not 'blue-sky' proposals but conservative projections based, first of all, on what it appears feasible actually to achieve and, secondly, on the 5-year targets that were approved by the Department last spring. If circumstances permit--that is, if financial constraints are relaxed--these reviews will provide a realistic basis for orderly program development in these areas during the next 4 or 5 years.

I should like to emphasize that these Federally-initiated programs are not being undertaken at the expense of the investigator-initiated research projects which have dominated NIH support up to the present time. They do not represent a change in NIH policy but an addition to NIH activities which should serve to provide a more effective approach to selected problems of major national importance that justify--or, indeed,

require--more organized collaboration and programmed activity than is characteristic of research conducted by individual investigators.

These Federally-initiated programs will not displace support for investigator-initiated research which must continue to be the main stream of biomedical research. The traditional research projects represent the consensus of the scientific community on what is important and what is feasible. This self-generated activity--with its mix of fundamental and applied research--will not only ensure the vigorous pursuit of the widest range of opportunities for progress but must continue to provide the scientific under-pinning for developmental projects and to point the direction for future targeted research.

The increased attention being given to these 'organized' programs should also not be interpreted as representing any wavering of our firm conviction about the over-riding importance of so-called basic research. In fact, the 'organized' programs of today have been made possible by the basic research of yesterday. Their present feasibility and probability of success is directly proportional to the soundness and completeness of the fundamental research on which they are based. It follows, of course, that the feasibility of future 'organized' or developmental programs will depend almost entirely on the scope and vigor of the fundamental research that we support in the intervening years. It is certainly true in scientific research that you can't harvest what you don't first sow.

Heart Transplant Problems

The difficulties that can be encountered as the result of gaps in fundamental research are now dramatically illustrated by the recent spate of heart-transplant operations. While the purely surgical techniques for such operations are already well-developed, too little research has, as yet, been completed on tissue antigens, species differences in rejection phenomena, and organ differences in rejection in the same species. A great deal more knowledge is necessary before any of these operations can become an accepted form of treatment. In fact, the newly established Board on Medicine of the National Academy of Sciences issued a formal statement on February 28 describing these operations as "a scientific exploration of the unknown" and urging that "only a relatively small number of careful investigations involving cardiac transplantation...be done at this time". At the same time, the American College of Cardiology called a conference--tentatively scheduled to start on March 23 at Bethesda--"to grapple with the problems arising from the development of techniques for heart transplantation". We are at present reviewing NIH activities on problems related to heart transplantation and considering how the necessary research can be accelerated, including relevant training and the provision of essential services. NIH should obviously take this task seriously and it will probably be necessary to redeploy some of our available funds to make a more concerted attack on these

problems. We are also giving urgent attention to the moral and ethical issues raised not only by heart transplantation but by a number of clinical investigations involving human subjects.

Fogarty Center

I am happy to report that we are making good progress in developing plans for the John E. Fogarty International Center for Advanced Study in the Health Sciences for which the Congress provided funds last year. We expect that a contract for architectual drawings will be awarded well before the end of this year. Meanwhile, we are making arrangements to renovate and remodel 'Stone House' which is the large and attractive residence of the former owners of part of the NIH campus. It sits on the site selected for the Fogarty Center and it seems to us to be both practical and prudent to incorporate it into the over-all design of the Center. As renovation can probably be completed in about three or four months, it will be possible to bring some of the Fogarty Center activities into being much sooner than if everything had to wait for the completion of the new building. We expect that the conference program and the scholars program can both be started early next year.

Budget Summary

The $1,196,693,000 requested for FY 1969 is distributed as follows among the five major activities of NIH:

... The $275.6 million requested for direct operations include $87.5 million for the conduct of research by the NIH scientific staff and $126.5 million for collaborative projects. The remaining $61.6 million is for other

direct operations, program direction, administrative services and management of the extramural program.

... Of the $652 million requested for research grants, nearly $165 million is for the special research grant programs--including $53 million for General Research Support--and $342 million is for non-competing research projects which we already have a commitment to support. This leaves $145 million for competing grant applications of which it is estimated that close to $91 million will be needed for supplemental awards for existing grants and for the renewal of projects to which continued support has not been committed. About $54 million will thus be left for the support of new grant projects --this is $6 million less than the amount appropriated for this purpose for FY 1968.

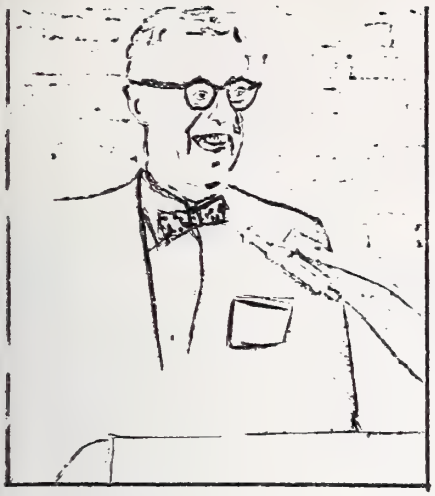
... The requests for training grants and fellowships total $197.8 million--an increase of $4.8 million over the FY 1968 appropriation.

... For Health Research Facilities Construction Grants, $8.4 million is requested. In addition, $12.2 million will be carried forward from the FY 1968 appropriation in accordance with the Bureau of the Budget's directive for curtailing FY 1968 expenditures.

... The request for the Regional Medical Programs is $62.9 million. This activity will also have a carry-over from FY 1968 of $30.9 million.

I believe that your review of these requests will confirm my earlier statement that this is as lean a budget as it is possible for us to present to you without a major cut-back in programs whose only purpose is to benefit the people of this country by strengthening the ability of the medical profession to diagnose, treat and prevent disease and disability.

TAB 1C1



533

French and Polyclinic Medical School
and Health Center Seminar

HEALTH CENTER OF THE FUTURE

- problems
- plans
- prospectives

JAMES A. SHANNON, M.D., *Chairman*

Seminar held at Hofstra University, Hempstead New York
April 15 + 16, 1968

Dr. James A. Shannon...

Health care has outgrown the ability of individuals to handle the problem, and society has not yet learned how to fill the void. Both the public and the medical profession are dissatisfied and are looking for new ways to deliver health care...

Until about ten years ago the problem of medical care was the special problem of the physician. This no longer pertains... Economists and sociologists probably have as much if not more to contribute than the individual practicing physician to the design of the systems and the design of the functional entities called hospitals which make up part of the system...

One could summarize by saying that health is no longer available to an individual through a single physician. He has to use some type of system as the only way he can get the benefits of medical advance... In terms of family physicians or private physicians, one must be guided through a maze of very complex procedures, involving multiple inputs from many physicians, in order to arrive at a diagnosis and provide treatment. Quite frankly, this may turn out to be not a physician but perhaps some specially trained social worker who can give the family emotional support and who can do something about some of the other things that trouble the family when illness strikes, and many of these things are beyond the competence of the physician.

James A. Shannon, M.D.
Seminar Chairman

TAB 102

PRESIDENT'S SCIENCE ADVISORY COMMITTEE PRESENTATION
BY Dr. James A. Shannon - April 18, 1967

Presentation will--

a. Define some characteristics of NIH as it has come to be

b. Relate this to some general problems

   Scientific

   Social

c. Attempt to define the plight of academia

d. Indicate why present proposals and present mechanisms are inadequate to solve what are important root problems

e. View the future Federal involvement in biomedical sciences--

   The problems

   The resources

   Reprogramming vs. extension

   Qualities of the extension

f. Return to institutions generally and the elements of a more general solution.

Problems--

1. Project system manageable--

   simplification desirable

   consolidation essential

   no combination satisfactory

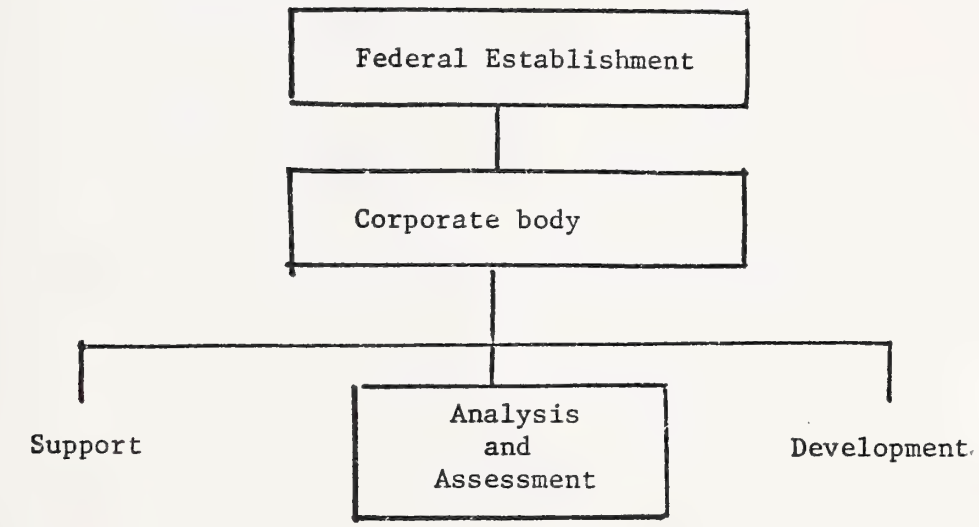
2. Institutional assessment less manageable

   Broad commitment could lead to Federal domination

   A new institutional setting

      Not NSF -- Not OE.

Possibly?



3. Unfinished business--

--The research institutes

New medical schools

Old medical schools

Special problem of dental sciences

Graduate education in biological and behavioral sciences

Broad innovation in professional education

--Areas of science

--Areas of social need.

4. Assessment of present status--

   Size and quality of enterprise in manageable terms

   The real needs--unmet

      Scientists

      Fields of science

      Institutions

   A realistic appraisal of the science mix fundamental, applied, development--concretely in each field and emphasis one field compared to another.

   A determination to satisfy needs at all levels by either

      Broad commitment with reprogramming of effort--radical of necessary

      Broad commitment with controlled expansion--hazzard

5. An initial appraisal--how are we utilizing our funds today--$1.0 billion

   a. by Institute

   b. Within an institute by activity

   c. Research in relation to program objective

   d. Distribution of effort

   Taking Heart Institute as an example.

6. Another appraisal--what is science base that we provide for the instructional process

   Support of science when medicine is viewed as an industrial activity--the gross sales $43 billion

   Support of science by institution

7. Conclude expansion desirable--

   What is base medical schools and geography

   1965

   1975

   Regional medical programs and distribution of science excellence as base for

   --continuing education

   --provide standards of excellence for medical care.

The major problems before us

1. Define the need

   Solution of disease problems and improve the quality of living

   Provide institutional stability for program

   Scientifically excellent

   Geographically dispersed

   Provides a suitable mix of fundamental and applied

   Evolves an institutional setting wherein both instruction and learning and research and science education will provide adequately for

   New Knowledge

   Physician production.

2. Assess the net cost and the modification of the Federal apparatus--two requirements
3. Provide for program selection (the choice)

Field to field--Space, Defense, Health

Distribution within field

Research, Science and professional education

Distribution within substantive areas of science

Distribution of effort

Mechanisms are not available to us that will provide--

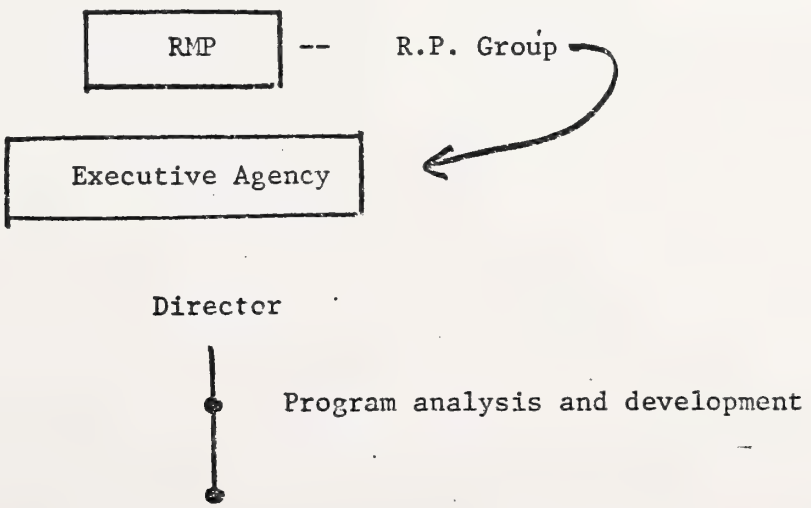
Massive support required to total field

Rational distribution of effort

Without Federal domination of total enterprise.

A Demonstration

1. Establish a not for profit corporation
2. Negotiate a program with Regional Planning Group



An institute

$4.0 million
66 professionals
10 groups of 6-7 scientists

Operating cost 1975

25	new medical schools	50 million
40	old medical schools	80 million
12	CV institutes	48 million
10	Cancer institutes	40 million
6	Dental sciences	24 million
6	Pharmacology & Toxicology	24 million
10	Behavioral sciences	40 million
6	Biomedical Engineering	24 million
		330 million

Others Aging

Environmental Health

Direct operation	20 million
10 centers (x 2.0)	20 million
Grants (x 200)	12 million
	52 million

Presentation

A one hour summary of what is a monographic view of

1. Development of NIH
2. Problems or "The crisis of Academia"
3. Some thoughts on solution

Time short--schedule crowded. House appropriation today.

This evening--group knows NIH but is not knowledgeable concerning the problems of which they are themselves a part FASEB

This morning--knowledgeable as to problems of academia not of role of NIH in its production

Presentation--

Magnitude of activity NIH--growth and development

Chart I Beginnings. 1890 Staten Island shortly to Washington

1st quarter of this century Hygienic Laboratory

--Distinguished Modern Biochemists

Clark, Hopkins, Connor NRC, Hastings Harvard

--Pioneering in nutrition—

Goldberger--Pellagra in South

--Infectious disease--

Epidemiologic developments--

Vaccine development with prevention

Modern Base--Establishment 1930-1950

NIH of 1930's

Cancer Institute 1937 -- basic authorities--

chronic illness a national problem

Postwar growth--

1944 - OSRD Contracts

1945-1950 - Beginnings of categorical approach to major chronic illness

Establishment of a broad direct operation--emergence of clinical sciences as primary object coequal with laboratory

1950-1955 - Completion of categorical structure

Development of administrative structure of Institutes--authorities and activities

1956-1962 -
- Period of rapid growth
- Role of Folsom--of Congress
- Problems of a laggard Executive Branch

Current program--

Direct operation

Collaborative research

Support of research--primarily by project

Development of resources

Resources are

- manpower
- laboratory space
- institutions.

Current programs — Biomedical Research

Magnitude -- R&D -- GNP

Medical -- all R&D

Medical -- source of funds

Medical -- performance

Character of program

-- a highly diversified program

research

training--in educational sense

facilities construction

institutional support

I. Establish the concept of the cognizant agency

Natural breaks--

Physical sciences and engineering	NSF
Health sciences	NIH
Arts and humanities	Other

Functions--Research and Education (graduate)

Program analysis and development

Other agencies not dependent states but strictly categorical

DoD	Interior
NASA	HUD
AEC	Transportation
Agriculture	Commerce

Office of Education broad responsibility to primary degree.

II. Establish for the Nation an equivalent to the Federal Reserve Board in finance to deal with science and the humanities insofar as these affect higher education and the general deployment of resources across all fields.

Non Executive in the Federal Service.

TAB 103

COLLEGE OF MOUNT ST. VINCENT
COMMENCEMENT ADDRESS*

James A. Shannon, M.D.**

While I am keenly aware of the significance of this occasion to every one of you, I have no message of profound significance. Rather, I have a few comments on a puzzling problem of today. I will try to make them simple and brief, for there is little that I could say that would enhance this proud and happy occasion.

C. P. Snow, the British scientist-turned-novelist, has highlighted the existence of "two cultures"--science and the humanities. Regretfully I must grant the truth of this--that there is a tendency of the well educated to separate into two camps, or perhaps two islands of thought and concern with few bridges between them. One important consequence is that science, in broad areas, is poorly utilized by the cognizant, articulate nonscientists of our society.

And yet science--

- Has the capacity to alter the human condition in radical ways--indeed, has already done so.
- Can only proceed at great public expense, which establishes the public as its major patron.

It must be said that science is not well understood by its patrons, even the better educated among them. It is viewed by many with cynicism or scepticism, by some as antithetic to true social progress.

---

*College of Mount St. Vincent Commencement Address, New York City, May 24, 1968.

**Director, National Institutes of Health, U.S. Department of Health, Education, and Welfare.

Such attitudes can have a devastating effect on the development of a nation. The situation may have been less corrosive during the early decades of the century, when the Nation's economic and industrial strength were still derived from the exploitation of physical resources. But today and in the future, a sound economic base for whatever we may wish to do as a nation will depend more upon how successfully we exploit our intellectual resources. It is the latter that must provide the cutting edge and set the pace of material advancement. But the educated members of both "cultures" are needed to guide and sustain the science-driven technologic revolution so as to utilize its product for sound social purposes. This will be difficult when all social institutions are themselves in rapid transformation.

Change is inevitable. But we must learn to be its master and not its servant.

A broader, more perceptive comprehension of science and its relation to the contemporary scene must be achieved by the society at large. This comprehension cannot have its source in news media at the headline and bulletin level, which tend to present science as a series of unrelated little spectaculars. Rather, it must be gained through the basic educational process, so that the interaction of science and society constitutes a subculture with its own guidelines and aspirations. In short, science must become absorbed in the liberal culture, where it can evolve as a part of, and not apart from, society.

Let me be more specific. Those of you who are oriented toward the sciences appreciate that science, after all, is but a method, a tool for probing the unknown. This tool is frequently guided only by curiosity,

and thus the yield is only vaguely predictable. In other cases, it is highly directed--a mix of fundamental and applied research quite relevant to specified problems. In either case, however, society's application of the knowledge derived from science will be made in the light of value judgments. One would hope that such application would reflect, to a high degree, our heritage of more than 2000 years of evolution in the humanities and their influence on the social order.

The scientist, then, in selecting his problems or participating in the application of new knowledge must weigh carefully the broad potential of his work in human, esthetic and spiritual terms, if he is to find complete fulfillment.

The obverse of this is equally true. The nonscientist must obtain a more sophisticated understanding of science, and the mechanics of science and its application, if society is to benefit maximally.

There will be many occasions in which a comprehension of some area of science will be important to you, and many opportunities to apply your background in the humanities to problems involving science, to the applications of science, and to its support. There is no doubt that science will have an increasing and all-pervasive influence in the further development of our civilization. The excellence of our science and its extension to technologic innovation will determine the Nation's capacity for industrial development, and consequently the level of disposable income. It will determine the level of health to which we may aspire. It will also, I am convinced, determine the degree to which we can comprehend society's options for solving many of its social ills.

I shall not dwell upon those ills. They will be the subject of many commencement addresses across the land, by speakers far better qualified than I. Suffice to say that much can be done with present knowledge, and that the limiting factors for the moment are the Nation's social purpose or purposes and its social determination. I shall only add that our remedies, to draw an analogy from medicine, are likely, through lack of sufficient information, to be more palliative than definitive. To pursue the analogy further, one might expect our initial remedies to be comparable to the 300-year use of chinchona bark to suppress swamp fever (vivax malaria with its repetitive relapses) and to have less in common with the modern prevention and treatment of malaria which followed factual knowledge of the cause and transmission of the disease.

But, be that as it may, let us come back to science and its present-day uses. This is of deep personal concern to all of you, for science and technology will have a profound effect on your lives. The bulk of science will undoubtedly be performed with your tax dollar, and the intimate relation of science to higher education has long since reached the point of no return.

Over the past two decades, both the physical and biomedical sciences have seen broad development in this country through support by Federal mission-oriented agencies. These agencies have sponsored far-ranging activities for purposes of better health, more productive agriculture, stronger defense, more effective aircraft, and the conquest of space. More recently, special agencies have addressed themselves to problems of

education per se; others, to problems of transportation, environmental pollution, the inner city, and the delivery of services (such as medical and social) essential to the operation of a very complex society.

These problems have a number of qualities in common:

- Each is of recognized importance to the citizenry at large, and so is national in scope.
- The solution of each will be expensive--so much so that judgments must be made as to the allocation of national resources.
- For each, there is a fund of information ready to be applied, and a decision must be made whether to use only the knowledge available, costly as this may be, or to seek further knowledge so as to permit a more definitive solution to the problem, or to support a program that contains a rational mix of further study and immediate application.
- Finally, every one of these major problems demands intimate participation of our universities if reasonable solutions are to evolve.

The generalities of these four propositions are accepted by both the legislative and executive branches of the Government. The questions posed in the execution of a program become--

- The scale.
- The allocation of resources.
- The mechanisms to employ.

How these questions are answered is of grave importance to society in general and to the institutions involved, particularly the universities. The interested, public-spirited citizen, applying his value judgments, will strongly influence the order of priorities and the force of measures undertaken. I shall return to the health field and make a few illustrative suggestions for an initial course of action.

If the general public were wholly rational and well grounded in matters of health, the individual would encounter no extraordinary difficulties in avoiding many major illnesses or obtaining excellent health care. There would be a more equitable distribution of medical services, even in the face of short resources.

This would permit intelligent application of scientific and technologic advances to problems of daily living. Diverse personal problems, ranging from such specific matters as weight control to the more general question of selecting medical aid, could be dealt with effectively. Problems of family and child development, of emotional, intellectual and biological growth, and of aging--in one's parents and later oneself--would also command rational approaches. Finally, we would support in our institutions a stable diversified program with a productive mix of research, education and service. But unfortunately, most persons--even you as college graduates--are medically uninformed. Despite the vital importance of health, most individuals are unprepared to deal with even the commoner personal health problems that may arise, much less address themselves to more abstract problems requiring national action.

The general lack of sound knowledge on health matters is a reflection of deficiencies in our educational system. Recent educational innovations include new mathematics, new biology, new chemistry and physics. Why not <u>new health education</u>? This could start in grammar school with material (now touched upon later) which would provide a general understanding of--

- Growth, development, maturation, and their basic imperatives.
- The mechanics and economy of the body organ systems and requirements, including biological and behavioral factors.
- Basic nutrition.

Secondary schools could present (in greater depth than at present) health requirements and aids in relation to disease and disability. A possible orientation of the curriculum could be--

- Child development.
- Problems of youth.
- Health maintenance for the young adult during maturity and beyond.
- Problems of aging.
- The ways and means of health research and systems for the delivery of health services.

The prebaccalaureate experience could then provide many interesting options. To mention one, there is the important and complex area of study relating health to the social problems of disadvantaged people. One could list many major problems that would illustrate the interactions of medicine, science and society.

But I would emphasize that health, to some extent, can be purchased with knowledge. Only people such as yourselves can correct this broad deficiency in the knowledge of the coming generation. Still within the health field--

- Those of you who enter the educational system can help directly in developing a stronger curriculum and a better array of texts and audio-visual aids.
- Those of you who enter public life can support educational reforms.

- Those of you who have children can inquire into deficiences of the educational system and can work through the PTA or local boards of education.

- All of you, as citizens, can urge support of a state and national program that provides a healthy mix of science education and service.

It will be many decades before a major portion of the anguish due to disease will be preventable, though this day will come. Meanwhile, much can be done. The impact of disease can be minimized, and many of its consequences controlled--but only by intelligent and forceful action.

In conclusion, I would again broaden the scope of my remarks. There is an urgent need to bring science and the humanities closer together. Indeed, the very survival of the human race may depend upon the intelligent and humanitarian application of present and future scientific knowledge. The steps to an adequate comprehension of science and its role in public affairs are not insurmountable, and there are many ways to proceed. In matters of health particularly--as a special case--there is great need for a general expansion of knowledge; but there is an even greater need for public education and the understanding that will follow.

So, as you leave here today, I hope you will take with you this thought: let your liberal education continue to be liberal, and become even broader as you continue to read and study. Involve yourselves in public and educational activities, and do not shy away from science and its application because of apparent complexity. Play some role in building the essential bridges between Snow's islands of science and those of the humanities. This will only be possible for a well-informed person, but the bridges are crucial to our national progress.

TAB 104

The General Electric FORUM

FOR EXPLORING THE FRONTIERS OF HUMAN PROGRESS

Volume XI • Number 1 • Summer 196

Table of Contents

A New Materials Age 4
Sir George P. Thomson,
author, physicist, Nobel Laureate
The relationship of materials to human progress.

How Can We Best Manage Our Resources? 8
Representative Wayne N. Aspinall (D.—Colo.),
Chairman, House Committee on Interior and Insular Affairs
Need for public and private commitment to achieve a balance between minerals preservation and use.

Special Section:
Meeting Today's Needs with Today's Materials 11
Introduction: Dr. Walter R. Hibbard, Jr.,
former Director, U.S. Bureau of Mines
Six leaders from industry, government, education, discuss materials needs and applications in:

Architecture 13
Walter H. Costa, Associate Partner,
Skidmore, Owings & Merrill

Medicine 15
Dr. James A. Shannon, Exec. Dir.
National Institutes of Health

National Defense 17
Dr. R. L. Sproull, Vice Pres., Academic Affairs,
Cornell University

Product Design 19
Professor Jay Doblin, Director, Institute of Design,
Illinois Institute of Technology

Energy Needs 21
Dr. Joseph L. Fisher, President,
Resources for the Future, Inc.

Communications 23
Dr. N. B. Hannay, Executive Director of Research,
Materials Division, Bell Telephone Laboratories

The Application of Discovery 25
Dr. Charles E. Reed, Vice President, Group Executive
Components and Materials, General Electric Company
Industry's role in materials development and application.

MEDICINE:
Improving the "Quality of Living"

interview with Dr. James A. Shannon,
Director, National Institutes of Health

Dr. Shannon has devoted his career to medical research, teaching, and public service. A graduate of New York University, he was a consultant on tropical diseases to the Secretary of War during World War II, for which he received the Presidential Medal of Merit. He was named head of the National Institutes of Health, the research arm of the Public Health Service, in 1955.

Dr. Shannon, what is the principal role of the National Institutes of Health in meeting national medical needs?

Simply stated, we are a national health agency—the biomedical research arm of the U.S. Public Health Service—concerned with the health and well-being of every U.S. citizen. Our primary purpose is to produce new biological and medical information. We do this in three ways: we conduct a broad research program directly in our own laboratories, support research within the university, and provide for the training of new scientists and the production of new resources. At present, we do not have a *direct* educational function from the standpoint of producing physicians. This is the responsibility of other parts of the Public Health Service.

Our present budget amounts to about $1.2 billion, and substantially more than 90 per cent is expended outside our own facilities in support of research and training. And the majority of this—some 80 per cent of NIH funds—goes directly to around 2000 colleges, universities and research institutions. In fact, NIH expenditures account for 55 per cent of the total Federal expenditure and 36 per cent of *all* U.S. funds for health-related research.

Didn't you state recently that the most critical goal of medicine and medical research should be to improve the "quality of living" and prevent untimely death, rather than to necessarily prolong life?

Yes. We are more concerned with preventing long-term disability than with just increasing the life span from, say, age 70 to 72. For example, neurological disorders, mental illnesses and mental retardation do not ordinarily kill, but they severely disable, often with much pain and suffering. I think that past generations have been overly preoccupied with tackling specific disease entities rather than with establishing broad comprehensive medical goals.

At NIH, we are more concerned with those elements of developmental process—intellectual, emotional, or biological—that lead to a child's maturing into full adulthood and to maintaining his health throughout productive life without serious disability.

This is what I mean by improving the "quality of living"—in the intellectual and emotional as well as the biological sense.

Perhaps your eight separate "Institutes" within the National Institutes of Health are indicative of your areas of emphasis in improving the quality of living, but aren't most of these categorized by "disease" areas—for example, institutes of heart, cancer, dentistry, arthritis?

They are, in a way, but actually this categorical structure stems from a different purpose. One cannot talk to the general public in terms of science *as* science, but rather we must emphasize how science can be used to prevent or cure disease. This goal is readily understood by Congress and by the people. In other words, if we cannot interpret the end mission of our Institutes in terms that are easily understood by the public that supports us, then we are asking for support of rather a blind nature.

But we do have two fairly recently established institutes not labeled by disease per se: the National Institute of General Medical Sciences and the National Institute of Child Health and Human Development. These reflect our substantially broader interest in the normal human development, as well as maintenance of health rather than preoccupation with specific diseases. And our most recent Division is Environmental Health Sciences, which again reflects interest in the quality of living.

How does the need for better or new medical materials —for example, modern synthetic materials for organ and limb replacement, tissue repair, and heart assist devices —fit into all this?

Such materials—plastics for blood vessel replacement, heart prosthetic devices, and the like—have been important, but perhaps not on as large a scale as other items which might equally be called medical materials. Under a broad definition of "materials," you could place almost anything that is mass-produced by industry and used in medicine.

Under this definition, I would include any bacterial substances—antibiotics, for example. One ordinarily thinks of antibiotics as having revolutionized the treatment of infectious disease, which indeed they have. But what is not well recognized is that antibiotics have been the essential tool that has permitted development of a whole array of viral vaccines. These have had a radical influence on all surgical procedures, even permitting cardiopulmonary, or

". . . the challenge to the medical community is to effectively apply and refine existing materials."

open-heart surgery to become a reality.

Again, a broader definition of materials would encompass synthetic organic compounds which have revolutionized the handling of hypertension, allergic disorders, and a wide variety of functional conditions that heretofore have been totally disabling.

So my description of medical materials would be those that are derived from the fine chemical industry as well as the heavy chemical industry. Under this umbrella then, materials have had, and are having, a fantastic influence on health and the field of medicine.

I gather you don't feel the medical community is particularly "materials limited" under this definition?

Put it this way. I don't think that new "materials" pose one of the major ds of medicine at this point in time. ..ather, the challenge is to effectively apply and refine existing materials. The big medical advances of the future will come from a broader understanding of the nature of disease, with more effective application of existing materials perhaps helping us to achieve such understanding.

Certainly there are areas where materials advance would be helpful—improved adhesives for surgical purposes, better definition of bonds between rock-like materials that might relate to bone mending or artificial tooth implants, substances to solve clotting problems in vascular surgery. The whole area of implantable heart assist devices represents an area of materials needs. Here we are dealing with the problem of body rejection of foreign materials. Much already has been done, but there are opportunities for further development in this area.

How do you view current heart transplant interest and activity?

Cautiously and conservatively. We try to avoid crash programs, to take on projects that will have a truly long-lasting benefit to the public, and to do so in a manner which makes the most effective use of public funds.

In 1964 judgment was made by one of our advisory groups that total replacement of the heart—utilizing a synthetic artificial organ—should be given one of the highest priorities of our program.

It was my conviction, and that of a number of our scientists here, that understanding of the biological processes was insufficient to justify an all-out effort. We decided to adopt a more limited approach, seeking to determine how far we could develop a *partial* heart assist device which in turn would aid in determining how really feasible a totally replaceable heart would be.

Meanwhile, we felt that we didn't have enough knowledge to effectively predict when to use a replacement heart even if we had one. For example, after a heart attack, why do a certain percentage of people die immediately, others in 24 hours, others in 48 hours, and still others survive for an indefinite period of time? These are the kinds of questions that must be accurately answered before we attempt heart replacement on any but the most hopeless cases. So it became important to establish also an extensive and definitive examination of those situations that might benefit through heart assist devices.

Such studies must go on in parallel with studies on heart assist devices so that we will know (a) whether heart assist devices are all that are needed, in which case our problem is relatively simple, (b) if total replacement is required, and if so, the nature of the replacement, and (c) what criteria we should use for such replacement?

Although our current five year program is somewhat more limited than was originally advised, it is more realistic. Whereas the original proposal would have been fantastically costly with no certainty of useful results, the present program will lead to a firm base for a later program for total heart replacement, should this be deemed necessary and truly beneficial to society.

What about the even broader ethical decisions we're going to face as heart studies and other medical research become more and more capable of extending life? Will, or should, the public become more directly involved in such decisions?

We are quite concerned that these very complex questions be called to the attention of not only the physician or the medical community, but to the public at large. Very simply, when devices such as artificial heart implants become more available to extend or protect life, selectivity as to recipients is going to have to be made. We already face that dilemma with the artificial kidney. Should recipients be individuals in certain age groups? Should they be people responsible for major support of a family? For the foreseeable future, the availability of such devices will be only in limited numbers.

As open-heart surgery becomes more common, costs will remain high—somewhere in the area of $1000 to $10,000. If the individual can't pay, what is to be the basis for whether or not he receives treatment? Should we expend effort on an individual with a life expectancy of only six months or who also has had multiple cerebral accidents? If it becomes possible in the next 20 years to manipulate genes, what new moral and ethical decisions are we going to face?

It is crucial that these broad problems be considered as they mesh—or fail to mesh—in our society at large. Thus, as I said before, the ethical questions of the future must be faced by all segments of our society.

"Medical advances of the future will come about from a broader understanding of the nature of the disease." National Heart Institute chemist classifies blood specimen.

NIH programs aim at improving the "quality of living" and preventing untimely death, rather than necessarily prolonging life. "We try to avoid crash programs, to take on projects that will have a truly long-lasting benefit to the public."

TAB 105

Statement by
James A. Shannon, M.D.
Director, National Institutes of Health

prepared for hearings before the
Subcommittee on Science, Research, and Development
House Science and Astronautics Committee
July 1968

Mr. Chairman and Members of the Committee:

The time has arrived, I feel, for a thorough reassessment of the relationship that has developed between the Federal Government and our institutions of higher education.

In the areas of science and science education, which concern us most in this series of hearings, this relationship has been described as a "partnership"--a particularly fruitful partnership. And quite clearly, a pattern of growing interdependence and mutual support can be traced back to World War II. For both partners, the contributions made and the advantages gained have been obvious.

The Government partner has defined a large number of socially important problems and supplied substantial funding toward their solutions. These societal needs find expression in the programs of mission-oriented Federal agencies that look to science for definitive answers in health, defense, space, transportation, and other areas. The academic partner, on the other hand, contains much of the Nation's science capability and, in a highly productive manner, utilizes this resource both in problem solving and in the production of the scientists and technologists so increasingly needed in our rapidly evolving technologically

based society.[1] For academic science institutions, Federal support for science now represents roughly a quarter to a third of all income available for their diverse purposes.[2] Today, because of the successes of this partnership, we are measurably healthier and maintain better control over nature in a better understood world.

Yet there are unmistakable signs that the arrangements which have served so well in the past are inadequate for the future. The rate of Federal investment in research and scientific development has slowed sharply, at a time when research costs and institutional enrollments are climbing steadily. Weaknesses in the system which are quite fundamental in nature have become apparent, and these go beyond the simple, though important, availability of money. Reduced quality and insolvency are hazards that have become very real for many science institutions. Thus we must now consider carefully how best to proceed.

Because of these convictions, Mr. Chairman, I welcome the opportunity to join with you, with other Committee members, and other science spokesmen, to discuss what can be done to strengthen our institutions of higher education and to secure for our academic-science partner the prospect of establishing the essential stability which is requisite to a continuation of our Nation's science effort.

1/ The essential role of industry as the third party in this relationship, though fully recognized, will only be treated incidentally in this presentation.

2/ Derived from data made available by the Office of Education, covering 88 publicly and 58 privately controlled institutions.

A focus for this discussion is provided by the proposal introduced by Chairman Miller of the House Science and Astronautics Committee: H.R. 875, a bill "to promote the advancement of science and the education of scientists through a national program of institutional grants to the colleges and universities of the United States."

I have some specific thoughts on that bill. I should like, however, to provide a meaningful context for my remarks by offering first an assessment of the present state of health in academic science.

What are some of the signs of a need for reassessment of the critical interdependence that has developed between Federal programs and academic institutions?

One is the general reduction of Federal funding for academic science after more than a decade of rapid or moderate expansion. This slowing, which affects both the performance of research and the training of scientists and engineers, invalidates many of the planning assumptions that have been made by academic institutions. Support through the various Federal programs is not keeping pace with emerging science opportunities or with the need for scientific manpower conservatively estimated to be essential. Nor are we adequately supporting the scientists we have helped to produce.

The extensive Federal investment in expanding the Nation's resources for academic science, through construction of facilities and support for training in a variety of research careers, is paying off as anticipated.

As a result, substantial numbers of competently trained scientists are now seeking the research opportunities for which they were trained. But not all who seek are finding, because of the unexpected reduction in Federal funds for research.

A brief mention of the main facts will suffice to support the point. There has been a pronounced slowing of the rate of increase in support since fiscal year 1965, quite apart from the severe curtailment we anticipate in fiscal year 1969.[3/] In the health area, for example, we now face an unfortunate coincidence between the maximum number of newly available biomedical scientists (some 7,000 estimated) and, quite apart from inflated costs, an absolute reduction of grant funds available for the support of research. The House appropriation action provides sums for fiscal 1969 which would permit the National Institutes of Health to make awards to less than one out of seven new applicants for grant support, and further reduction is anticipated as a result of executive action.[4/]

I do not describe this very difficult situation in criticism of budget allocations or other judgments on national priorities. On the contrary, within fiscal constraints deemed to be prudent by the Congress, we are being treated about as well as could reasonably be expected. But

---

3/ Between 1962 and 1966, according to the National Academy of Sciences, 39,920 Ph.D.'s were awarded in selected science fields. Federal support for academic sciences increased about $1.8 billion in 1965 and nearly $2.2 billion in 1966, although the rate of growth of Federal support of academic sciences has declined sharply. (National Science Foundation, Federal Support to Universities and Colleges, FY 1963-66.)

4/ Research grant funds in the NIH budget recommended by the House for fiscal 1969 are slightly below those for 1968. When account is taken of increased costs (4%-8%), the reduction will amount to between 5% and 9%. This is prior to any downward adjustment to satisfy the requirements of P.L. 90-364.

the point I would make is that the effects of those constraints on academic science reveal basic deficiencies in our arrangements for guiding the national science endeavor.

We have created a setting wherein a substantial portion of this Nation's resources for research and graduate education is incidentally dependent upon the pursuit of various social goals by a number of Federal mission-oriented agencies. We as a Nation have made no commitment to maintain either the quality or the viability of an essential academic enterprise at any given level that can be related in a meaningful fashion to national objectives or to the quality and volume of intellectual talent essential to our further scientific and technologic development.

Furthermore, we have failed to provide, in even a primitive fashion, the elements of a unified overview of academic science. There is a central point of science responsibility in the office of the President. But there is no mechanism that permits effective planning for long- and short-term consequences of the responses that Federal agencies must make to a shortage of funds in the coming year. The Office of Science and Technology is striving to do these things, but without adequate staff or authority. It seems certain to many of us that, lacking a suitable central mechanism with the responsibility, capability, and authority to act in the overall national interest in these matters, we must expect our academic establishment to face perennial crises related to the varying budgetary fortunes of the many supporting agencies.

There are other signs that our basic organization for science is deficient. Certainly we know too little about the real needs and problems of the academic science institutions with which we do business. Each supporting Federal agency is necessarily preoccupied with the problems of its own programs, and this is proper. It is not proper, however, that we should encounter an unplanned, sharply shrinking academic base for science which occurs almost overnight. It is not proper that apprently healthy departments or even whole institutions of substantial excellence suddenly find themselves in such difficulty that they cease to function effectively. Nor is it proper that the precipitating causes of these improprieties are individual program decisions within the multiple Federal agencies concerned with the support of the academic enterprise. Blame for these failings must be placed at the door of the system and not at that of the harried Federal and university officials involved.

It is true that many otherwise competent science institutions still have poor accounting systems, plan unrealistically or not at all, and effectively hide their real financial and staffing worries from Federal program managers. But it is also true that there is no Federal mechanism or point of responsibility for concern with the health of institutions as institutions, much less with the Nation's aggregate science enterprise. Nor is there a mechanism for assessing the impact of Federal program decisions on particular institutions or on the aggregate effort.

This is true despite an elaborate framework for coordination among Federal programs. For the most part, this formal and informal framework relates to the substance of science and agency performance in specific areas. It rarely deals with the generalities of science or institutional viability. Nonetheless, we frequently proclaim that our institutions of higher learning are an indispensable national resource.

Federal program decisions are made--each independently of the other or nearly so--by the hundreds of individual program managers or special review groups that award Federal funds. And Federal program managers and review groups, under existing statutes, have as their main obligation the identification and support of the fields, institutions, and individuals that promise the most toward accomplishing the agency's mission. The welfare of academic science institutions as a whole comes farther down the priority list and, in fact, is only lightly funded by several programs of the National Science Foundation, National Institutes of Health, and one or two other agencies.

Our present framework for the support of academic science inherently contains, to a dangerous degree, a basic instability--despite the real need for some sense of operational stability of institutions providing education and research activity. Academic support is too much a simple adjunct of national research and development enterprises. It is too frequently provided in the form of support for a particular project selected on the basis of its excellence and relevance to the mission of the supportive agency. The institution is gravely affected by fluctuations

in the budgetary and appropriation processes of the several agencies involved in support of its scientists and their programs. In fact, the academic partner has become to an alarming degree the servant of the Federal establishment, without benefit of the institutional equivalent of either unemployment insurance or social security.

This situation, I believe, has now brought us to the point where a new look at the relevance of certain precepts long accepted as governing Federal-university relationships is warranted. For example, we need to re-examine in a contemporary context the heretofore accepted value of diversity and variety in the Federal sources of support for basic academic functions. Would institutions be better or worse off if they dealt with fewer agencies--or, in some instances, possibly only one--in trying to meet support needs and to develop their fullest potential?

We will continue to need mission-oriented science support and free access to academic institutions. Of this I am sure. But I suspect that we might serve institutional needs better, and make more efficient use of limited resources for science, with a somewhat less decentralized approach.

We need to ask ourselves also whether the project support system has over-reached its point of maximum utility as the principal, indeed almost exclusive means to develop a sound science and education base for the Nation. While the project system has served us well--and it will continue in any case to have a major support role as an objective, equitable, and productive means for advancing specific national objectives--it seems

clear that it will require broader supplementation by other mechanisms, conducted within a more cohesive framework, than has been tried so far. We need to consider what those mechanisms should be and what portion of our total resources should be allocated through them. The National Institutional Grants program proposed in H.R. 875 is one possibility to be explored in this context.

I will mention just one more problem area that indicates to me a need to reassess what the Federal Government is doing in relation to its academic science partner. This Committee, as well as others in both houses of the Congress, has urged a more equitable geographic distribution of Federal research funds and therewith the development of additional centers of academic-science excellence to meet the needs of all national regions. The President, too, has endorsed these goals, and has directed agency action to attempt their achievement. Despite wide agreement on the desirability and importance of the latter objectives, progress has been disappointing and, indeed, with the present mechanisms available, little further progress is to be anticipated.

The principal difficulties include the lack of any real national commitment to solve the many problems easily perceived and the lack of an overall strategy for solving them, including provision of the necessary resources. Our national science establishment is not organized to deal effectively with the institutional issues involved. We have yet to define what "equitable geographic distribution" means in terms of specific objectives--short- and longer-range--including numbers and distribution of institutions at both

the undergraduate and graduate levels. Obviously, then, we have no plan as yet for achieving such objectives. I would add that, in most discussions on this subject, we tend to overlook the difficult but essential task of sustaining competence and excellence where they are now found. Finally, we have yet to agree on precise means to identify either "competence" or "excellence" in academic science institutions.

On the Federal side, our principal handicap--apart from the lack of an overall strategy--is that there is no real operational locus for Federal concern with institutions as such. Such a locus would seem to be a prerequisite for significant progress.

At local community and individual institutional levels, there has been too little realism in the identification of objectives and the development of plans to achieve them. Not every institution can or should aspire to academic-science excellence in the broad and highly differentiated sense attributable to a small number of major university centers. Competence in essentially all departments, graduate and undergraduate, and excellence in one or two at the higher level, represents a valuable and sufficiently challenging objective for most institutions.

Individuals planning the development of their institutions to new levels of competence or excellence must be realistic in their assessment of the strengths and advantages on which they are attempting to build. The characteristics of both the institution and the general community environment are important. Substantial non-Federal funds, over and above

those already provided, must be made available--generally through tax revenues. Difficult choices must be made locally among the institutions competing for development.

What must be called into being, I think, is a series of regional coordinating groups--working within a national framework--representing regional and not Federal perceptions of need. These groups will have to make the hard choices--that is, to set short- and long-range regional objectives, to identify the local institutions warranting development (and to what level), and to mobilize the substantial investment of local resources required.

Finally, we must sort out what needs to be done in the following quite different tasks:

(1) the sustaining of excellence and competence in academic-science institutions where it is now found,

(2) the promotion of academic-science excellence in one or more departments of institutions with unusual promise, and

(3) the development of a broad academic-science competence in additional institutions.

The priorities for each of these tasks will need to be set.

While thus providing in an intelligent way for the strengthening of our academic-science capabilities in an institutional sense, I would stress that we must also provide a continuing mechanism for "doing the Nation's business" through a project system that reflects the legitimate requirements of mission-oriented agencies.

With this perspective on the nature of the problems facing the partnership between the Government and academic science institutions today, I should like to move now to a consideration of H.R. 875 and the national program of institutional grants it proposes.

I fully agree with the basic premise of H.R. 875--that colleges and universities have a need for funds that can be used with broad discretion by each institution to meet its particular needs and priorities. I recognize also that the bill is prompted by the critical situation facing higher education.

Nevertheless, I do not feel that our highest priority for action should be the elaboration of yet another plan for broad distribution of moneys to colleges and universities, as H.R. 875 proposes. The more fundamental problems that I have discussed so far--the need to identify our national objectives with respect to academic science institutions and to reorganize Federal efforts to make those objectives attainable--should have first claim on our attention. Without careful identification of objectives and realistic assessment of feasible approaches, the resources likely to be available for institutional development will probably not make too much of an impact in the direction we all desire.

On the other hand, since broader devices for institutional support will undoubtedly be a part of whatever Federal strategy is worked out, I should like to identify certain elements that trouble me in the formula proposed by H.R. 875.

(1) While we do not know precisely how the benefits of the bill would be allocated among different kinds of institutions, the bill's formula would probably provide larger benefits for the larger and often stronger institutions while failing to meet the needs of smaller and weaker institutions or the junior colleges.

Two-thirds of the funds would be paid out on the basis of project support and advanced degrees awarded, factors which will clearly direct the bulk of the funds to the larger or more successful schools. And to the extent that one objective of the bill is to strengthen smaller or weaker institutions, it is highly doubtful that the proposed support program, which would provide funds for virtually all institutions of higher education and give them broad discretion, would be effective in achieving this objective.

(2) Since the bill would distribute one-third of the funds among States on a population basis, we assume that one purpose of the bill is to provide for the development of a more even geographic distribution of strong institutions. As I have pointed out, however, it is doubtful that either the relatively small funds provided or the formula distribution approach could promise much progress.

(3) Insofar as scientific fields are concerned, the bill does not take account of the modest institutional grant programs which are playing a valuable role in broadening the capabilities of already strong institutions. These programs include the Sustaining University Program of the National Aeronautics and Space Administration, the Institutional Science

Program of the National Science Foundation, and the General Research Support Grants Program of the National Institutes of Health. To support these programs during fiscal year 1968, a total of $193.2 million was requested in the President's Budget and appropriations of $156.7 million were received.

(4) The fields covered by the bill are the physical, biological, and social sciences, and engineering and mathematics. "Biological" might be changed to the more inclusive term "biomedical." Still, the fields are so many and so broad that the bill might be better described as providing a truncated program of general aid (the most outstanding omission being the humanities) than as promoting "the advancement of science and the education of scientists."

Conclusions

Mr. Chairman, it seems to me that a moderate tinkering with support mechanisms will be inadequate to the broad challenges ahead. What is needed is a radical restructuring of the Federal establishment as it relates to institutions of higher education.

It will be necessary to reassess some of our most cherished shibboleths, cliches, and traditional postures. These include primary dependence on project-support systems, the contemporary value of support through multiple agencies, and the traditional disinterest of the scientist in the condition and fate of his institution.

New academic organizations and new institutional forms may be needed to house parts of the emerging science of the future.

We will have to reconsider the establishment of a major Federal focus for concern with institutions of higher education, in their totality, and with their role in the achievement of national purposes. New arrangements will be needed for the exercise of full Federal support functions.

I should like to spell out two suggestions:

First, we have suggested to the President's Science Advisory Committee, the Office of Science and Technology, and indeed to the Secretary of HEW, that perhaps the time has come to designate, quite formally, particular agencies as "cognizant" over a given area of science. Then both the executive branch and Congress could look to one focal point--the "cognizant agency"--for data on the science area, for analyses, and perhaps for general recommendations as to the adequacy or inadequacy of support. The establishment of a limited number of cognizant agencies to supply information on the distribution of scientific effort, on needs, and so forth--rather than to direct the total research effort--would be acceptable, I think, to most executive departments.

Such "cognizance" does not exist today, either for science generally or for specific science areas. It is my belief that the Office of Science and Tehcnology is not adequately staffed to take on this very large burden in its general form; and I do not feel that NSF is a suitable agency to do this.

While we do not have such a series of cognizant agencies now, this will almost certainly be required in the near future. Let me be specific.

I hope that other agencies which impinge on our activities will not feel that I am presumptuous, but I believe that NIH exerts such a dominant influence on the biomedical sciences that it would be the logical agency to serve as the cognizant one in that field, or perhaps more broadly in the "life sciences."

I think the National Science Foundation could best serve as the cognizant agency for the physical sciences, mathematics, and engineering.

The cognizant agency for the social sciences might appropriately be either the Office of Education or--if it is established--the new foundation proposed by Senator Harris and others.

One might wish, in order to provide balance, to develop a fourth area--and to designate an appropriate cognizant agency--that would deal with the Arts and Humanities.

My second suggestion is that, in setting up these cognizant agencies, a mechanism be established, at a very high level in the Government, to provide overview and to guide national policy development. This might be a Board--perhaps at a level comparable with that of the Council of Economic Advisers, the National Security Agency, or the like--that would concern itself with the development of national goals and priorities for science and with the role of institutions in their achievement. To be truly effective, such an apparatus must also bear the Federal establishment's responsibility for the vitality and needs of our academic science institutions ad institutions, both individually and collectively. This

concern need not be limited to science aspects of these institutions, but would extend to the full mix of research, education, and associated activities.

This Board, with the information given to it by the cognizant agencies, would be able to assess whether the Nation was indeed making suitable progress toward identified goals. It would also be in a position to recommend steps to improve general rates of progress where needed.

Such a broad coordinating function has not come into being at the Federal level, nor is it likely to with our present structuring.

Without proposing a specific mechanism for carrying out this function, one might mention certain possibilities. The range includes a non-Federal coordinating group (on the pattern of the Federal Reserve Board). It could be of mixed Federal and non-Federal make-up, like the Council of Economic Advisers; or it could be a new Federal agency--perhaps one created through combining the present staff functions of OST, certain of those of NSF, and those of PSAC as the base for a new operational entity. I am convinced that the agency should not itself be concerned with operational programs.

Mr. Chairman, I would add one further point: that in the aggregate, the functions of setting goals for science, technology, and education; of assessing progress toward these goals; and of determining whether it is indeed in the national interest to do this or that in terms of broad priorities, could well be the most important consideration to which we should address ourselves. I believe that specific proposals to strengthen the academic science capabilities of our institutions of higher learning can only be rationally viewed within such a context.

TAB 106

#554

Delivered by James A. Shannon
Director, NIH; 4:00 p.m.
8/27/68

Hail and Farewell

I thank all of you for being here this afternoon. In a few days I will be leaving these surroundings which have become so familiar over the past nineteen years. And as I look back over that space of time, I find that there is one characteristic of those swiftly passing years which stands out above all others. That is change.

In 1949, when I joined the National Heart Institute, NIH was a much smaller physical plant. We had only about ninety acres at the beginning of the year but by July three major land acquisitions had brought the reservation to its present size. There were only five Institutes back then, two of which bear different names today.

After I became NIH Director in 1955, the changes continued. Two new Institutes were created. Five of the six Divisions we now have were added. NIH acquired land in Poolesville and opened its Animal Center there.

Since 1955, the NIH Appropriations have risen from $81 million to the current $1.2 billion--an increase which has permitted the evolution of NIH into a focal point for biomedical research progress, and the greatest single source of funds for support of medical research both in this country and abroad.

In these thirteen years, the NIH staff has more than doubled, from 5,400 to more than 13,400 today. Eighteen new buildings or major additions have been constructed. I remember a couple of years ago speaking in terms of NIH needing to provide some fifty percent more laboratory space for its scientists. Today, we have visible proof of the near achievement of that goal under our present building program. The NCI Virus Isolation Facility, Building 41, over near the National Library of Medicine, and the NCI-NIMH/NINDB complex facing Old Georgetown Road are nearly ready for occupancy. Conspicuous and nearing completion is the new extension to Building 31 and earlier this year, we had a modest expansion of the Clinical Center.

The ~~Extramurally, our~~ rate of growth of the extramural program has naturally been even greater ~~has been even more exponential~~. In 1950, medical research accounted for only 5.5 percent of the $2.9 billion spent on research and development in the United States. Today, with total R&D amounting to $25 billion, medical research now accounts for 10 percent of this total. The support of NIH has been a major factor in the broadening of the medical research endeavor. NIH accounts for more than a third of such research support today, but its undoubted influence in spurring increased effort by private and academic sectors must also be taken into account.

Such statistics and comparisons are convenient ways to sketch the ~~tremendous~~ growth of NIH over the last few years. Yet, growth alone is not a measure of an institution's success, particularly an institution of government. We all know, too well, the criticism that government grows too much, and in so doing becomes inefficient, ineffective--a bureaucracy in the worst sense. Growth, in order to be justified, must occur in answer to a pressing need for such expansion. The further development of existing programs, and the instigation of new methods and mechanisms when required, are legitimate responses to the presentation of new challenges and new opportunities for advancement.

I am proud that the growth of NIH has been of this character. And it is not in any mere deference to this assembly when I say to my immediate staff that what I have been able to accomplish during these thirteen years of growth has been possible in a large measure because of each of you. And at this time I would also like to thank all those who do the bureaucratic chores and who do them faithfully and well--the secretaries, the typists, those who run messages and deliver the mail, those who drive the cars and those who maintain the grounds. All of you are citizens of the NIH and each and everyone of you

performs a vital function; without the performance of those duties this vast organization would be brought to a halt.

In these thirteen years NIH has, I believe, been accepted by the scientific and academic communities as proof that Federal scientists working in Federal laboratories can become major contributors to the advance in medical progress. Many have become leaders in their fields. It is, of course, true that some portion of this success can be attributed to the excellence of our facilities and the availability of advanced research equipment. But the major credit for this acceptance of NIH must be given to our guardian[illegible]

k

tl

pr

be [illegible] base of knowledge vital to the national welfare, at the same time allowing for a minimum of Federal direction or control.

This year, as a result of the Departmental reorganization, NIH has enlarged its scope of concern to include the development of medical manpower resources, and the classification and dissemination of biomedical information. These new responsibilities, added to our traditional ones, will further stimulate our organization. There is no doubt in my mind that all of you will help accomplish these tasks--as in the past--boldly, imaginatively, and competently.

Viewed totally, the NIH mission is a social mission. It is with much satisfaction that I survey NIH's past and with much optimism that I look toward the future. It is a future about which even the best informed among us can only speculate. A definitive understanding of the biological basis for human development, the clarification of the complex problems of disease leading to both cure and prevention are among the final pay-offs for all of our programs. Stated more simply and more in sociological terms, our objective, in the final analysis, is better health for our citizens. Under the leadership of such men as Dr. Robert Q. Marston, your new Director, and others in medical research, education and practice, America will come into an era of health and wellbeing unparalleled in the history of mankind.

Walter Lippmann once said of President Franklin D. Roosevelt, "The final test of a leader is that he leaves behind him in other men the conviction and the will to carry on."

There is no question of the conviction and the will of you who are here today. And if I have been able to contribute something to the deepening of your convictions, to the strengthening of your wills, then I consider my service here well spent. But NIH has always required more of its staff than these qualities--admirable as they are. It has always required of its staff the same professionalism and qualifications that it has demanded of the scientists whose work it supports. And this is important, because, in the last analysis, we are not supporting Science but scientists--and our efforts will be measured by the excellence of our research results which are directly proportional to the excellence of our scientists. There has never been any compromise with excellence, at NIH, nor, I am sure, will there be any compromise in the future. This uncompromising "hard line" is the only line consistent with the goals we seek, the mission we have to fulfill.

Thank you.

TAB 107

Reprinted from the Transactions of the Association of American Physicians
vol. lxxxi, 1968

DINNER ADDRESS

THE UNIVERSITY MEDICAL CENTER AND MEDICAL EDUCATION*

JAMES A. SHANNON†

THE development of modern medical centers, begun at the turn of the century in a few universities, soon became a discernible trend. In 1910, there were 66 approved medical and basic science schools. The number stood at 77 throughout most of the 1930s and '40s, and by 1953, there were 80.[1] Today there are 85 "university" medical centers,‡ if the term is roughly defined as:

> A fully accredited four-year medical school,
> Rendering community service usually beyond the immediate demands of teaching,
> Conducting health-related research of fundamental or applied nature, and
> Providing a diversified program of advanced education at the postdoctoral level.

Some of the schools are associated with a university, others are not. And no judgment is made as to the quality of the individual enterprise.

The Science Component

One of the valid measures of progress in the development of these medical centers is the research content of the program.[3] Measured in dollars available, 83 medical schools reported a total expenditure of $75 million for sponsored research in 1957. In 1966, 87 reported a total of $514 million—nearly a 7-fold increase. This sum represents 43 percent of all expenditure by the schools, exclusive of patient-care costs.

Between 1962 and 1967, there was a doubling of the number of students enrolled in graduate programs in basic science departments—from 4811 to 8459.[4] Over that same period, there was also a doubling of PhDs awarded in (selected) basic sciences—from 331 to 632. Last year, in addition, 4186 fellows and postdoctoral students were enrolled in graduate degree programs in the clinical sciences, with 47 PhDs and 422

* Adapted from a presentation before the Association of American Physicians, Atlantic City, N. J., May 7, 1968, when the author was Director, National Institutes of Health, U.S. Department of Health, Education, and Welfare.

† Special Adviser to the President, National Academy of Sciences.

‡ Includes the University of New Mexico School of Medicine, which graduated its first class this June.[2]

masters degrees awarded. While still low, these figures on clinical-science degrees represent another upward trend.

Government Support of Science

We might take note here of the Federal Government's growing role in the development of medical research, one of the principal attributes of the medical center. The support of all medical research in the Nation (including industrial, but excluding training and construction) was about 2 parts non-Federal to 1 part Federal in 1947, about 1-to-1 by 1953, and is now 2 parts Federal to 1 part non-Federal.[5] Over that time, total dollars expended nationally for medical and health-related research have risen from $87 million to $2.5 billion. The Nation's medical schools derive from Federal sources 54 percent of their total expenditures and 82 percent of their expenditures for sponsored research.[6]

Significant numbers of physicians are conducting or studying medical science under training grants and fellowships of the National Institutes of Health.[7] In fiscal year 1967 about 6900 MDs were receiving stipend support under the training grants and about 1000 under the fellowships. Fifty-six hundred of the MDs were in clinical fields. Medical schools had over 12,000 NIH trainees and about 1200 NIH fellows.*

Beginning in 1962, programs parallel to the science activities in these institutions were initiated by the Federal establishment. The first was directed toward increasing the rate of physician production. Others that followed were directed toward the utilization of medical centers as a resource to facilitate the development of an array of functions whose primary thrust was the delivery of health services. With these and other added functions, the medical center has now become a full partner with the Federal agencies in the achievement of many diverse missions. This association implies a vital interest of the Government in the affairs of the medical center and a derivative responsibility for its vigor and effectiveness.

The main determinant of vigor and effectiveness would appear to be the quality of the center's leadership. Next in importance, I would say with deep conviction, is its administrative and financial stability. To a very great extent, the continued strength and independence of the medical center will depend on this, and the means for ensuring such stability are not beyond command.

Strengths and Weaknesses of the Centers

In surveying the role of the medical center in the Nation's medical system today, I can agree with much of Dean Turner's summary before the AAMC in October 1966:[8]

* Includes medical and other health-professional schools, full- and part-time participation, and all types of degree and discipline.

> The great university medical centers of America . . . have become the critical links in the health program of the country in at least two vital respects. First, the university medical centers are the principal source of health and medical manpower of the nation; second, for each community in which located, they constitute focal points of excellence in medical practice, points from which improvements in preventive and curative medicine are most likely to stem.

But this understates the contribution of the university itself in the production of health personnel, including medical scientists and educators, and that of a substantial number of extraordinary community hospitals as focal points for excellence.

On the negative side, I *cannot* share the view, seemingly more and more prevalent, that the typical university medical center constitutes a highly integrated universe capable of unlimited expansion; that it stands at the pinnacle of excellence in all important respects, with its only major need an unlimited access to funds; and that every center must be organized along the lines of Harvard, Hopkins or Chicago.

The typical university medical center, in my view, is an organizational disaster—cumbersome, administratively chaotic, and unstable to a high degree. As an operating entity, it too frequently lacks clear lines of responsibility and authority, with each professional performing multiple functions. It tends to install departments as more or less autonomous units; offers too little in the way of reasonable patterns for career development; is difficult to fund, though rising costs impose an increasing need for support. And its basic instability, not yet manifest in the younger programs, will have mounting significance—a point which I will discuss more fully later. In fact, one could compare the typical university medical center to a hotel in which each guest can build a wing of his own and demand service.

It is with some nostalgia that one looks back at the simple structure and simple function of these centers in the pre-World War II period. There, as portrayed in Figure 1, modest programs of research, instruction and service mutually reinforced one another, with the objectives being largely limited to the production of physicians and the conduct of undifferentiated research. Funding of such enterprises, though always difficult, was uncomplicated as compared with today.

The modern university medical center, depicted in Figure 2, bears little relationship to such a simple structure. It still has a central responsibility for the production of physicians and the performance of undifferentiated research. But these are now only small programs when viewed in relation to the aggregate. Figure 2 reflects the general manner in which each of the initially reinforcing functions (research, instruction and service) has been expanded with its individual goals. These programs enrich the general environment of the institution, but may have little relevance to its unique function—the production of physicians.

THE MEDICAL SCHOOL

1940

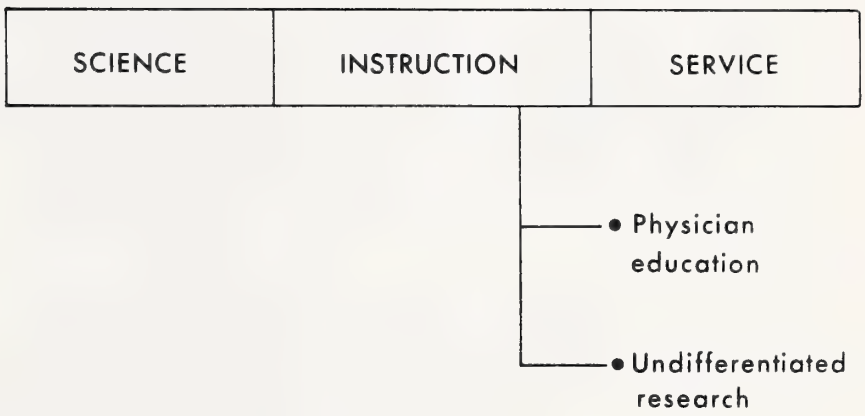
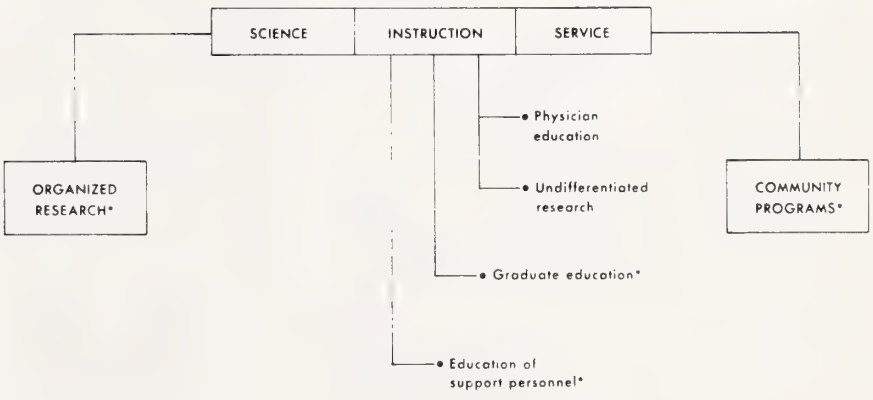


FIG. 1

THE MODERN UNIVERSITY MEDICAL CENTER

1967



*Broad Federal support

FIG. 2

There has been added:

Graduate education to yield PhDs in biomedical sciences, MDs with postdoctoral training in science and the medical specialties, opportunities for the continuing education of physicians, and greatly expanded programs of residency training.

Extensive education of nurses and other auxiliary health professionals and technologists.

Organized research, conducted largely in response to national aims and objectives. The organizational units range from complex but medium-sized departmental programs to full-blown research institutes.

Community programs, largely goal-oriented and peripheral to what has been historically the primary function of the medical school or center. I have in mind the extensive programs in mental health and mental retardation, the community health activities of the Office of Economic Opportunity, and the involvement of the medical faculty with the operations of the Regional Medical Programs, Comprehensive Health Planning program, and the Center for Health Services Research.

Increasing preoccupation with the stability of these complex institutions, particularly during the past two or three years, has led me to ask several deans to define the portion of the total enterprise that warranted stabilization and the magnitude of the funds that would be required. I have had few responsible answers. Most were to the effect that the entire enterprise required stability on the one hand and unim-

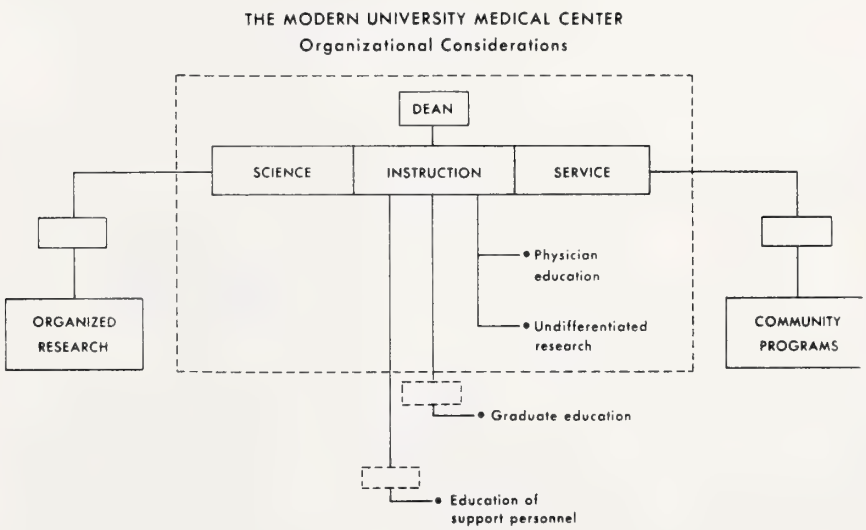


FIG. 3

peded opportunity for growth on the other. Yet we can clearly see that the universe defined by this figure is not amenable to definition and is capable of unlimited growth.

Moreover, I am told that medical schools no longer exist as such and that we must adjust our concern to the totality of the medical center. This, despite the fact that the only unique attribute of the center is the production of physicians.

I am convinced that these are unreasonable answers and provide no adequate basis for planning.

Figure 3 superimposes a dotted-line frame over the previous diagram, isolating the functions essential to medical education. In an abstract way these might be called the "core" activities of the medical school. Certain administrative entities are also shown as boxes: the dean and his staff, the heads of research departments and community programs. Evolving offices concerned with directing programs of graduate education and ancillary training are sketched in. It is the administration and support of these activities that I now wish to emphasize.

Formation and Administration

Is it possible to combine the elements of excellence of a modern medical center while avoiding the demands, and the risks, of more formal organization? If so, what are the nodes about which such an organization could be developed? The possibilities are—

Undergraduate education (involving a limited mix of research, instruction and service),
Graduate education,
Research (apart from that which relates intimately to undergraduate teaching), and
Community service.

It is conceivable that stable organizations might be built around any or all of these program segments.

In any case, however, the school as an educational institution must have a central administrative structure. This should provide—

Direction,
Planning and evaluation,
Central services,
Stable funding for whatever are considered to be core activities.

The core activities of the medical school—namely, those that relate to its only unique function, education of the physician and such programs as render him maximally effective, including some research, service, and graduate study—must be treated as a continuum. But their essential purpose should be recognized as that of providing a basic educational experience in preparation for the many options that lie ahead for the young physician. In this age of science, one prerequisite to the various

careers possible is certainly a broad science base. Another, in an age of social change, is a better understanding of the social matrix containing the health care system.

The core function can be adequately discharged only if well defined, stably financed, and capably directed. Support of this function from outside—and no one denies that much is needed—must derive from authorizations and programs reflecting some order and definition on the part of the schools. With a view to sound organization and management, we must define the core's program, staffing, tangible assets and liabilities, fiscal resources, and results. In this light, activities exterior to the core will be seen to enrich the curriculum and present discrete career options. And they will be amenable to independent financing in their own right.

Yet this operation must not be divisive in the sense that it subverts the vital interplay between core and peripheral activities which constitute an extension of the social and science base essential for development of the graduate student. Nor should it result in a rigid, line-operated structure, with such compartmentalization as to stifle initiative and exchange of ideas. On the contrary, identification of the core of the medical school can lead to better institutional planning, direct and impartial support, and strengthening of all the school's activities.

Present and Potential Support

If the medical schools are willing to review their activities along the lines proposed, the Federal Government could be expected to respond with appropriate supportive action, even though this action would involve substantial change in the administrative attitudes of the Federal agencies.

An inventory of support programs now extant is quite revealing. These include:

Organized research programs—fundamental, applied and developmental. This is the area of the greatest potential for growth of research support over the next decade.

Undifferentiated research under the research project system, fellowships, and training grants. This activity is guided by the scientific community's judgment of what is important, and must remain the keystone of federally sponsored research activity; but it is to be emphasized that this type of support is goal-oriented in the aggregate, with the Federal establishment setting both the goals and the priorities.

Research resource programs, such as those providing for construction of research facilities, general research and training support on a formula basis, and project grants for science development.

Programs that involve community activities with explicit social purposes, as named above. As is the case with the science oriented programs, these too enrich the environment of the center but still have their own goals which are quite peripheral to the unique mission of the institution.

Finally, there are educational programs of recent origin. These include construction projects, basic improvement grants, and project development grants. The present programs constitute a base for necessary extension and modification. With suitable support mechanisms and adequate funding, they can provide, in conjunction with the general research support programs, a sound base for total institutional development. In this view, educational support might be substituted for much departmental support of training activities, which should proceed in association with the extended research function.

Certainly, broad Federal support has not been lacking in the areas of high public interest—indeed, has encouraged the schools to extend themselves in diverse ways. Despite the serious efforts of Federal agencies to help the schools meet administrative costs of research, and to foster constructive over-all measures in support of science and social services, many schools have encountered difficulties more or less related to the explosion of secondary programs. This explosive growth is usually attributable to scientific pressure from the faculty, social pressure from the community, or opportunity as perceived to utilize new programs.

Quite valid concern for these important programs has focused attention on the fundamental instability of the present framework for support of the academic enterprise—an essential if the many major goals are to be achieved. But one is struck by the singular equation, the better the superstructure, the weaker the building. And above all, the unique function of the medical school—the education of physicians—has become submerged. That the core activities have resisted definition and consequently are not supported in a purposeful and adequate way has, in my view, contributed to the difficulties I have cited, and in fact jeopardizes the entire structure.

Yet broad support of the core activities can emerge as an important part of the university-Federal relationship. This depends primarily on the thoughtfulness and leadership derived from the schools themselves. It is difficult to define general needs. Each school is different, and the program in each case is a matter for internal judgment. But no school is altogether unique, though most make some claim to uniqueness. Actually, their similarities outweigh their differences.

In other words, each school has a central set of unique activities subject to some definition, to some accounting, and to some administration, and can be dependent to the extent necessary upon the Federal Government. The more precise the definition of these activities, the less the detail that will be required in accounting systems.

Assuming that appropriate authority can be obtained and adequately implemented, the medical centers will become an increasingly important national resource for personnel—research workers, teachers, administrators—and will thus be better prepared to satisfy a growing social need

Looking Ahead

I would summarize by saying that I believe medical education can look to the Federal Government for subsidy of a broad nature. But the schools must be prepared to offer wisdom and counsel as to how this, as only one of several competing major national objectives, can best be realized.

I trust that the future development of medical centers will be marked by a purposeful differentiation in objectives, organization, and program development. At present, most claim that they produce a basic physician by a fairly nonstandard process—through a mixture of science-education-service in a rather stylized four-year period. They really differ only in the amount of science and service they superimpose on the educational process and in the excellence of this process itself.

They claim a place, and rightly, as a "natural resource," primarily because they produce that socially essential product, the physician. They also strive now, above and beyond this, to satisfy some social need in the immediate community, and they perform research as a primary end. But in the very diffuseness of these objectives, there is potential weakness and confusion.

It is common to say that the medical center has evolved into a scholarly environment reflective of the best that our modern university can produce. This I truly believe to be the case for a number of individual departments of our great centers, but find difficulty in applying the term "scholarly environment" to the chaotic situation that prevails in most schools. To foster scholarship demands much more than a massive and largely undifferentiated research enterprise, wherein the paucity of primary science education is attested by the low output of science doctorates in contrast with that of the university.

The medical center must be prepared to play an increasingly important role in American medicine. Its value in education, research, and community service is already proven. But its role as a viable, superior institution for the education of physicians and scientists prepared to meet critical national needs, present and future, is jeopardized by financial instability and vagueness of aim. Thus its destiny may hinge upon the responsiveness of leaders in medical education to opportunities for Federal support of more sharply delineated structures and programs.

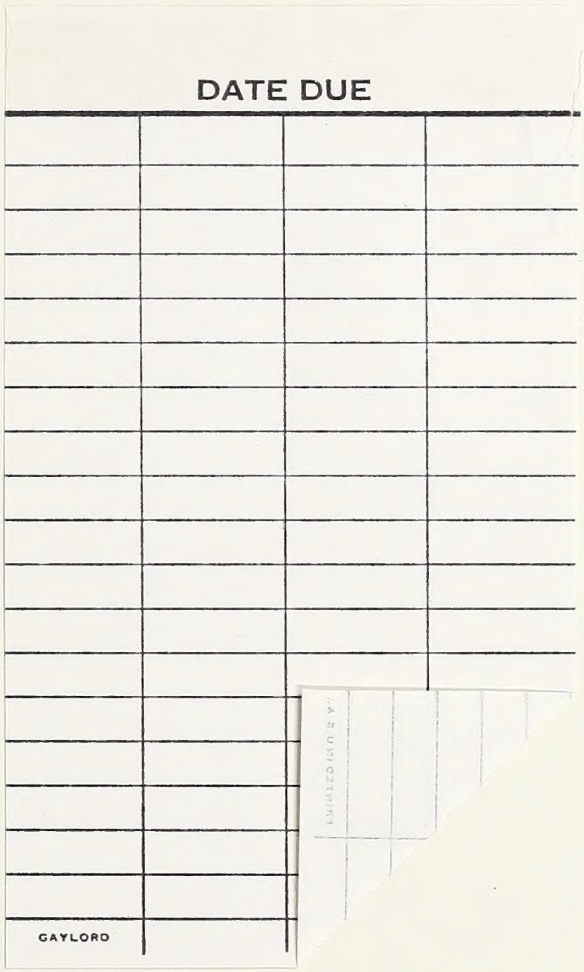
REFERENCES

1. American Medical Association. *Medical Education in the United States, 1966–1967*, p. 756, from the Education Number of JAMA, *202*; 726–832, November 20, 1967.
2. Ibid., p. 729.
3. Ibid., p. 758.
4. Ibid., p. 751.
5. NIH. *Basic Data Relating to the National Institutes of Health, 1968.*
6. AMA. *Op. cit.*, p. 758.
7. Division of Research Grants and Office of Program Planning, NIH.
8. Turner, Thomas B.: *The Medical Schools Twenty Years Afterwards: Impact of the Extramural Research Support Programs of the National Institutes of Health*, presidential address before the Association of American Medical Colleges, October 23, 1966. J. Med. Ed. *42*:109–118, February 1967.

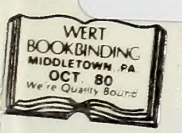
PRINTED IN U.S.A.

7591

NIH Library, Building 10
National Institutes of Health
Bethesda, Md. 20205



LIBRARY
NIH
Amazing Research.
Amazing Help.
http://nihlibrary.nih.gov
10 Center Drive
Bethesda, MD 20892-1150
301-496-1080



NIH LIBRARY
4 0144 3559

NIH LIBRARY
3 1496 00005 8100

NOV - 2 2000